Annual Book Issue

SCIENCE

27 April 1956

Volume 123, Number 3200

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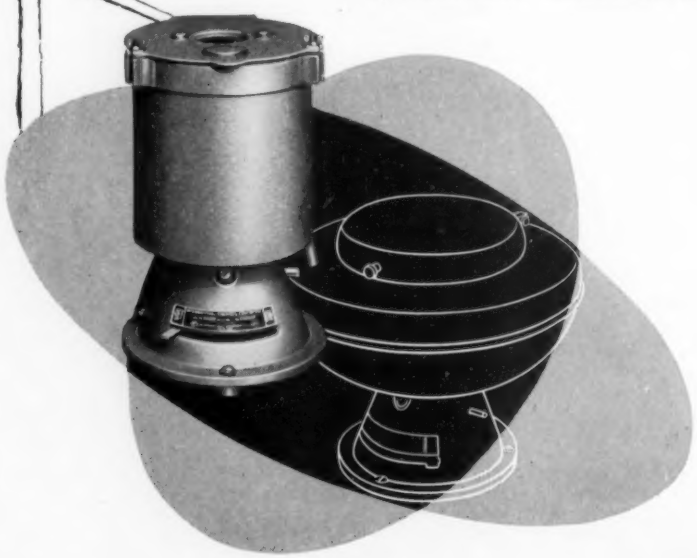

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Simple Sexual Reproduction
Reproduction by Seeds
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Part II. The Plant Kingdom

Classes and Names
Thallophyta: Algae
Thallophyta: Bacterial & Slime Molds
Thallophyta: True Fungi
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Tracheophyta: The Fern Allies
Pteropsida: Ferns
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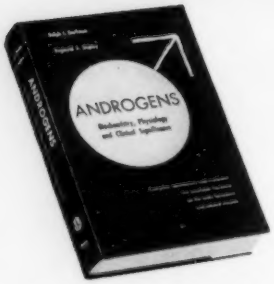

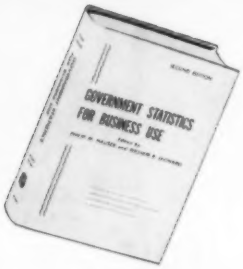

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Edited by P. F. Hahn, Cancer Research Laboratories, Nashville, Tenn. This practical account by 29 research specialists provides an authoritative guide to the treatment of virtually all benign and malignant conditions which are susceptible to handling with radioactive isotopes. May 1956. 414 pages. Prob. $10.00.

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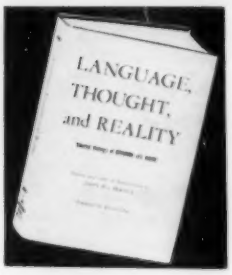

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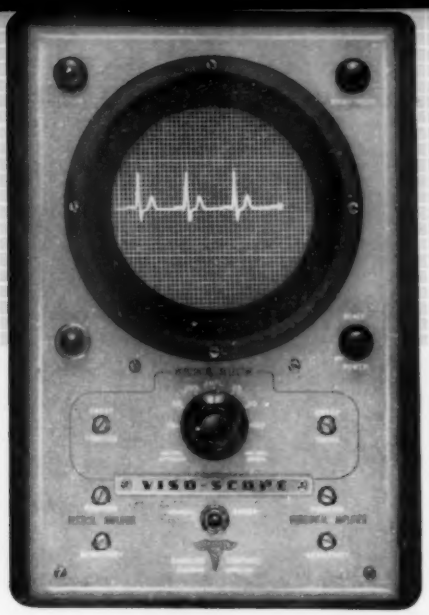

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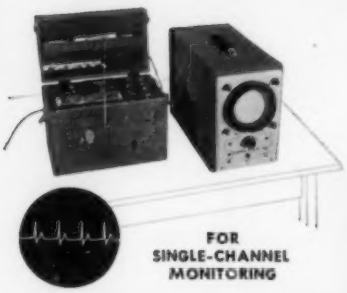

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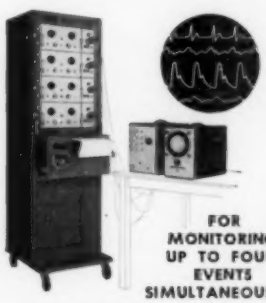

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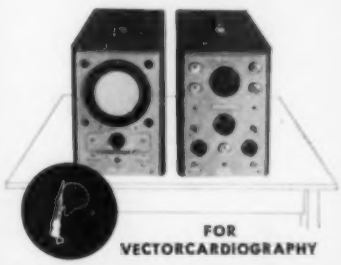

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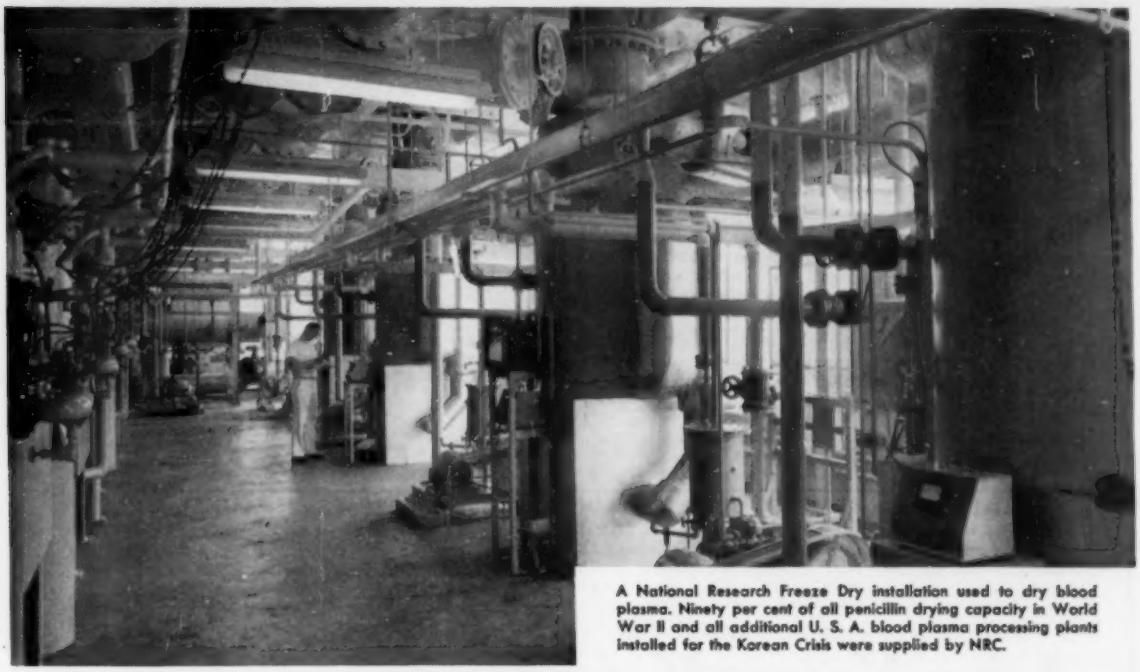

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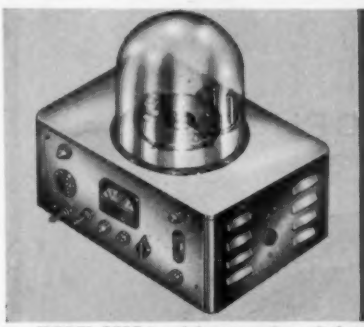

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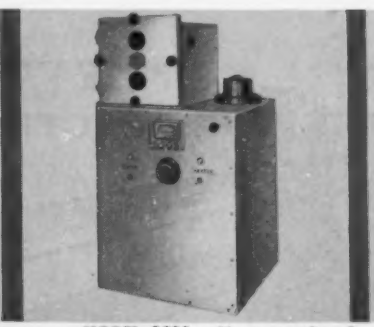

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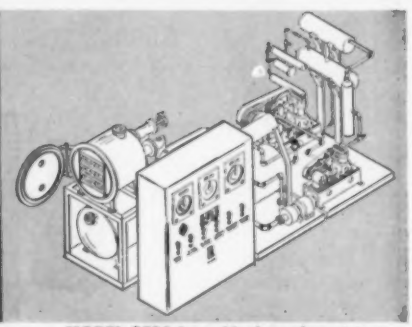

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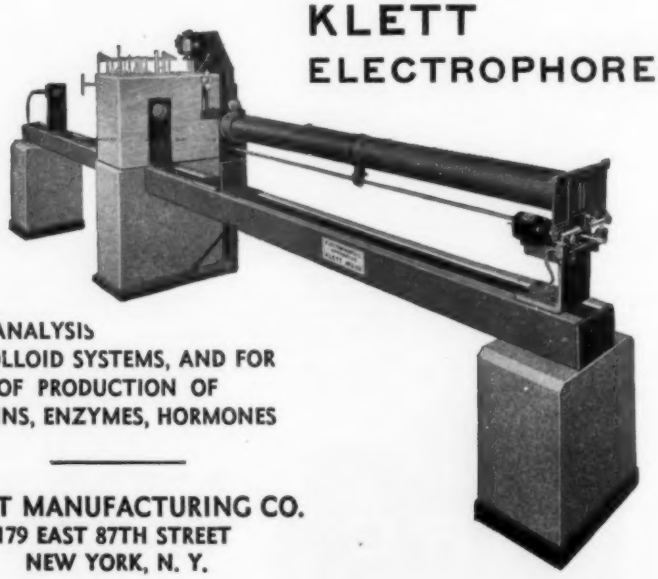

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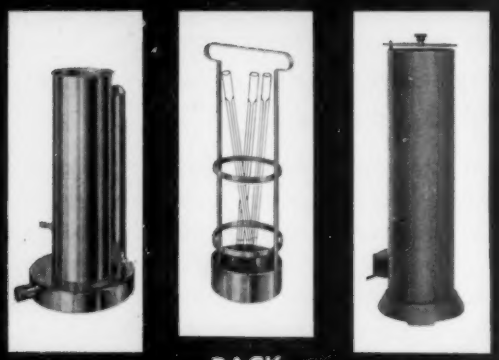

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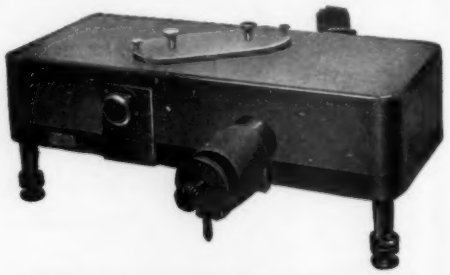

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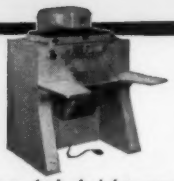

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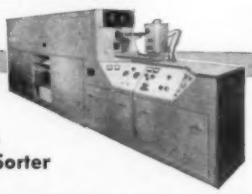

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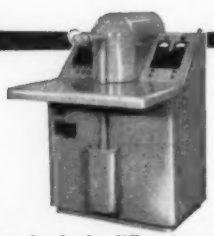

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Of Books and Reading

Last year a record 19,962 books were published in the United Kingdom and a near-record 12,589 in the United States. Whether we should regard the combined output of more than 30,000 books with pride or alarm is uncertain.

But perhaps the following two questions are more important: How well are the books assimilated? How many people read them? These questions can be answered, at least in part, thanks to the fact that for the last 20 years the American Institute of Public Opinion has been regularly surveying the reading habits of the American people. Through its affiliates abroad, the institute has gathered comparable information for several foreign countries.

In the United States no more than 17 percent of all adults were to be found reading books at the time of the most recent survey. This is in sorry contrast to the percentages in several other countries: Canada, 31; Australia, 34; England, 55.

The reading habits of our high-school and college graduates are likewise depressing: 57 percent of our high-school and 26 percent of our college graduates have not read a single book for the past year. A special study confined to college graduates showed that five out of six had not read a serious book outside their fields of special interest during the preceding several months.

We might hopefully suppose that, even though college graduates do not read much currently, they have some background of reading, some acquaintance with "the best that has been known and said." The results are again disappointing. The college graduates were asked to name the authors of the following books: *An American Tragedy, Babbitt, The Canterbury Tales, Gulliver's Travels, Leaves of Grass, The Old Wives' Tale, Utopia, Vanity Fair, The Origin of Species, The Wealth of Nations, The Rubaiyat,* and *The History of Tom Jones.*

Nine percent could not name the author of any one of these books and 39 percent could not name more than three authors. Of the same group of college graduates, only 55 percent could name any recently published book.

Perhaps the pocket-size books are filling the gap? Not so. Nine percent of the people who buy books of this kind account for 78 percent of all sales. Book clubs? Unfortunately, no. Although books clubs distribute millions of books annually to hundreds of thousands of readers, the effect is statistically unimportant.

Are there any indications of a change? Currently, as pointed out above, only 17 percent of all adults were reading a book at the time they were interviewed; the comparable figure in 1937 was 29 percent. The trend is apparently downward.

The statistics give evidence of a grave cultural inadequacy and an even graver cultural decline. Books, both new and old, play an important part, possibly an indispensable part, in the transmission of the ideas that hold a civilization together. The relatively small—and apparently declining—number of people in the United States who read books should be a matter for general concern. We have heard much about Rudolf Flesch's book, *Why Johnny Can't Read,* a book that has been widely discussed. Whether or not Johnny can read is one thing, whether or not he does when he grows up is another. The answer apparently is: seldom.—G. DuS.

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The Psychosemanticist Will See You Now, Mr. Thurber

James Thurber

I believe there are no scientific investigators that actually call themselves psychosemanticists, but it is surely time for these highly specialized therapeuticians to set up offices. They must not be carelessly confused with psychosomaticists, who study the effects of mental weather upon the ramparts of the body. The psychosemanticists will specialize in the havoc wrought by verbal artillery upon the fortress of reason. Their job will be to cope with the psychic trauma caused by linguistic meaninglessness, to prevent the language from degenerating into gibberish, and to save the sanity of persons threatened by the onset of polysyllabic monstrosititis.

We have always been a nation of categorizationists, but what was once merely a national characteristic is showing signs of malignancy. I shall not attempt to discover the incipient primary lesion, for I am not a qualified research scholar in this field. Indeed, for having had the impudence to trespass thus far I shall no doubt be denounced by the classificationists as a fractional impactionist (one who hits subjects a glancing blow), an unauthorized incursionist, a unilateral conclusionist, and a presumptuous deductionist. Our national predilection for ponderous phraseology has been traced by one authority as far back as the awkward expression "taxation without representation" (unjust impost). It is interesting to note that the irate American colonists of that period in our history would be categorized today as "anti-taxation-without-representationists."

Not long ago, for the most recent instance in my collection, Senator Lyndon Johnson was described by a Washington newspaperman as a pragmatic functionalist, a term that was used in a laudatory sense. It isn't always easy nowadays to tell the laudatory from the derogatory at first glance, but we should be glad that this Democratic leader is not a dogmatic divisionary or an occlusive impedimentarian. The most alarming incidence of verbal premalignancy occurs, of course, in this very area of politics, but let us skip over such worn and familiar double-jointedisms as creeping Socialists, disgruntled ex-employees, ritualistic liberals, massive retaliationists, agonized reappraisalists, unorthodox thinkers, unwitting handmaidens (male), to name only a few out of hundreds, and take a look at excessive prewar anti-Fascism, a colossal (I use the adjective as a noun, in the manner of television's "spectacular") that was disgorged a few years ago. Here the classificatory degradationists brought a time element into what might be called the post-evaluation of political morality. The operation of this kind of judgment during and after the Civil War would have thrown indelible suspicion upon all the Northern patriots, including Abraham Lincoln, who wanted Robert E. Lee to take command of the Federal Armies in the field. They would be known today as "overenthusiastic pre-Manassas pro-Leeists."

The carcinomenclature of our time is, to be sure, an agglomerative phenomenon of accumulated concretions, to which a dozen different types of elaborative descriptivists have contributed—eminently the old Communist intellectuals, with their "dialectical materialists," "factional deviationists," "unimplemented obscurantists," and so on, and so on. Once the political terminologists of all parties began to cross-infect our moribund vocabulary, the rate of degeneration became appalling. Elephantiasis of cliché set in, synonym atrophied, the pulse of inventiveness slowed alarmingly, and paraphrase died of impaction. Multiple sclerosis was apparent in the dragging rhythms of speech, and the complexion of writing and of conversation began to take on the tight, dry parchment look of death. We have become satisfied with gangrenous repetitions of threadbarisms, like an old man cackling in a chimney corner, and the onset of utter meaninglessness is imminent.

The symptoms of this ominous condition show up most clearly in the tertiary stage of "controversial figure." The most complicated specimen of this type of modern American is the man of unquestionable loyalty, distinguished public service, and outstanding ability and experience who has nonetheless "lost his usefulness." Actually, this victim of verbositosis has not lost his usefulness, his nation has lost it. It doesn't do the national psyche any good to realize that a man may be cut off in the full flower of his usefulness, on the ground that that is not what it is. I trust I have made the urgent need for psychosemanticists apparent, even though I have admittedly become contaminated in the process, and I doubt whether my own psychosemanticist, after treating me, will ever be able to turn to my wife and say cheerfully, "Madam, your husband will write clearly again."

Before visiting my hypothetical psychosemanticist for a brief imaginary interview, I feel that I should get something reassuring into this survey of depressing ailments of the tongue. We have, then, cured, or at least survived, various incipient mouth maladies in the past. There was a moment when "globaloneyism," growing out of the Timethod of wordoggle, seemed likely to become epidemic, but it fortunately turned out to be no worse than a touch of pig Latin or a slight case of Knock, Knock, Who's There? Congress was not prepared to adopt the telescoping of words, which takes both time and ingenuity, and unless an expression becomes absorbed by Congressionalese, it has little chance of general survival. This brings me to what may easily be the direct cause of my being bundled off to the psychosemanticist's before long: the beating the word

Mr. Thurber is a well-known cartoonist, writer, and playwright. This article is reprinted by permission from *The New Yorker*, 26 May 1955. Copr. 1955 The New Yorker Magazine, Inc.

"security" is taking in this great, scared land of ours. It is becoming paralyzed. This is bound to occur to any forceful word when it loses its quality of affirmation and is employed exclusively in a connotation of fear, uncertainty, and suspicion. The most frequent use of "security" (I hate to add to its shakiness with quotation marks, which have taken on a tone of mockery in our day) is in "security risk," "weakest link in our chain of security," and "lulled into a false sense of security." Precision of speech and meaning takes a small tossing around in the last of those three phrases. "Lulled" is actually what happens to a nation after it has been argued, tricked, maneuvered, reasoned, coaxed, cajoled, or jockeyed into a false sense of security, but the inflexibility that has descended upon us has ruled out the once noble search for the perfect word and the exact expression. What Eric Partridge calls "a poverty of linguistic resource" is exemplified by the practically exclusive use of two verbs in any public-forum discussion of national security. It is threatened or it is bolstered; I never heard of its being supported, reinforced, fortified, buttressed, or shored up, and only very rarely is it menaced, endangered, or in jeopardy.

The word "insecurity," by the way, seems to have been taken over by the psychiatrists as their personal property. In politics, as in penology, "security" itself has come to mean "insecurity." Take, for example, this sentence: "He was considered a 'maximum security' prisoner because of his police record and was never allowed out of his cell block." Similarly, "security data" means data of the kind calculated to scare the living daylights out of you, if not, indeed, your pants off. I could prove that "'maximum," in the case of the prisoner mentioned above, really means "minimum," but I don't want to get us in so deep that we can't get out. The present confused usage of "security" may have originated with the ancient Romans. Anyway, here is what Cassell's Latin Dictionary has to say about *securitas*: "I. *freedom from care*. A. In a good sense, *peace of mind*, quiet, Cic. B. In a bad sense, *carelessness, indifference*, Tac. II. Transf., *freedom from danger, security*, Tac."

A vital and restless breed of men, given to tapping our toes and drumming with our fingers, infatuated with every new crazy rhythm that rears its ugly beat, we have never truly loved harmony, the graceful structure of shapes and tones, and for this blindness and deafness we pay the awful price of continuous cacophony. It gets into language as well as music; we mug melody for the sake of sound effects, and the louder and more dissonant they are, the better we seem to like them. Our national veins have taken in the singing blood of Italy, Wales, Ireland, and Germany, but the transfusion has had no beneficial effect. Great big blocky words and phrases bumble off our tongues and presses every day. In four weeks of purposeful listening to the radio and reading the newspapers I have come up with a staggering list, full of sound and fury, dignifying nothing: "automation," "roadability," "humature," "motivational cognition" (this baby turned up in a series of travel lectures and was never defined), "fractionalization," "varietism," "redesegregation," "additive," "concertization" (this means giving a concert in a hall, and is not to be confused with cinematization or televisionization). The colloquial deformity "knowledgeable," which should have been clubbed to death years ago, when it first began crawling about like the late Lon Chaney, has gained new life in recent months. It is a dented derby of a word, often found in the scrawny company of such battered straw hats as "do-gooder," "know-how," "update," "uptake" (I recently uptook the iodine uptake test for thyroidism), and others so ugly and strange I can't decipher them in my notes. One of them looks like "de-egghead," which would mean to disintellectualize or mentally emasculate—a crippling operation approved by an alarming number of squashheads, in Washington and elsewhere.

During my month of vigil and research, I heard an able physiologist who has a radio program say, quite simply, "We do not use up all the food we take in." He wasn't allowed to get away with that piece of clarity, however, "Ah," cut in his announcer, for the benefit of those no longer able to understand simplicity, "the utilization factor!" I turned from this station to a droning psychologist, just in time to hear him say, "The female is sometimes the sexual aggressor." Here a familiar noun of mental illness and military invasion was clumsily at work beating in the skull of love with a verbal bung-starter. The sweetheart now often wears the fustian of the sick man and the Caesar. In the evening, I tuned in on one of the space-patrol programs that gleefully exude the great big blockyisms. "Your astrogation bank will tell you!" cried the captain of a space ship to another interplanetary pilot, meaning his navigational instruments. In a fairy tale, an astrogation bank would be a "star panel," but the quality of fairy tale is nowhere to be found in these dime novels of the constellations.

One Sunday morning, my head aching with "kiss-close" and "swivel-chair-it," meaning, I guess, "at kissing distance" and "maul it over in your executive brain," respectively, I stumbled upon a small radio station that had been captured by a man of God, ominous and squealful, who was begging his listeners to live on their knees, not as slaves but as supplicants. This particular fundamentalist, or maybe it is fundamentalitarian, had probably never heard of the great protest "I would rather die on my feet than live on my knees." But these yammering eschatologists, and many of their followers, have even less respect for the glory and grace of English than the unsaved politicians. "Let us cease to sugar-coat, let us cease to whitewash, let us cease to bargain-counter the Bible!" the speaker implored us. He finished second in vulgarity, I regret to say, to a reverend I had heard earlier in the year, who shouted, "I didn't cook up this dish, God cooked it up. I'm just dishing it out to ye!" The line between holiness and blasphemy becomes even thinner when some of the lay testimonialists begin ranting. "I own a shoe store in New Jersey," one of them confessed, "but Jesus Christ is my senior partner."

A recent investigation of the worries and concerns of five thousand selected Americans revealed that we are preoccupied almost wholly with the personal and private, and are troubled only mildly by political anxieties, including the danger of war, the state of civil liberties, and the internal Communist threat. This does not come as a surprise to me, since the nature of our national concern about Communism is proved to be personal by such expressions as "anti-anti-Communists" and "anti-anti-anti-Communists." The first actually means men who are against men who are against Communists, and the second, when you unravel it, means men who are against men who are against men who are against Communists. In these wonderful examples of our love of formidable elaborationisms, concept and doctrine are put aside, and personalities take their place. What we have left is pure personalism—a specific reactionary who is against a specific liberal who is against Senator McCarthy, let us say. The multiplicity of prefixes, another sign of linguistic poverty, was touched with a fine and healthful irony in Quincy Howe's invention of the phrase "ex-ex-Communist." (Many will claim that for their own, but Mr. Howe got to it first.) One would think that Americans would be worried, or at least concerned, by a man who may have ceased to be a man who may have ceased to be a Communist, but the Worry Research I have mentioned showed that this isn't so. We are worried about health, family matters, and money, and we have no time for a man who may be lying about lying. Incidentally, a fairly new advertising slogan, "The portable portable," fits neatly into modern jargon: the typewriter that you can carry that you can carry.

While I was exploring the decline of expression in America, I spent a week

in a hospital. Medical science has done much for humanity, but not in the area of verbal communication. It should undergo a prefectomy, and have some of its prefixes taken out. I should like to see the "semi" removed from "semi-private," a dispiriting word that originated in hospitals; there must be a less depressing way of describing a room with two or more beds. I am also for taking the "sub" out of "sub-clinical," and starting all over again with the idea in mind of making the word mean something. Incidentally, I discovered at the hospital the difference between "to be hospitalized" and "to become hospitalized." The first means to be placed in a hospital, and the second has two meanings: to get so that you can't stand it in the hospital any longer, and to like it so much there you don't want to leave.

Lying in bed brooding over these matters, I turned on the radio and heard an American describe another American as "an old-time A.D.A. type of anti-Jeffersonian radical"—a beautiful specimen of bumblery. Sir Winston Churchill in the exhilarating years of his public life, turned out many phrases as sharp as stilettos—for one example, "squalid gamin." But you can count on your fingers the Americans, since the Thomas Paine of "the summer soldier and the sunshine patriot," who have added bright, clear phrases to our language. If you can bumble an opponent to death why stab him seems to be the general feeling among our politicians, some of whom have got through the ten years since the war ended with only five adjectives of derogation: naïve, hostile, unrealistic, complacent, and irresponsible. All these slither easily, if boggily, into bumblery, and the bumbler is spared the tedious exercising of his mental faculties.

The day I got dressed and was about to leave the hospital, I heard a nurse and an interne discussing a patient who had got something in his eye. "It's a bad city to get something in your eye in," the nurse said. "Yes," the interne agreed, "but there isn't a better place to get something in your eye out in." I rushed past them with my hair in my wild eyes, and left the hospital. It was high time, too.

When and if I find a reputable psychosemanticist, I want to take up with him something that happened to me one night more than two years ago. It may be the basis of my etymological or philological problems, if that's what they are —words, especially big ones, are beginning to lose their meaning for me. Anyway, I woke up one summer night, from a deep dream of peacelessness, only to realize that I had been startled by nothing whatever into a false sense of insecurity. I had a desperate feeling that I was being closed in on, that there was a menace in the woods behind my house or on the road in front of it, watchful, waiting, biding its time. A few weeks later I bought a .38-calibre Smith & Wesson police revolver, which startled my wife into a genuine sense of insecurity. She hid the gun somewhere, and the cartridges somewhere else, and I still don't know where they are. I have often thought of telling my psychosemanticist about it, and I sometimes have the feeling that I did call on him and that the interview went like this:

"Doesn't your wife's hiding the gun worry you?" he asked.

"No," I said.

"It would me," he confessed.

"It would *what* you?" I demanded.

It seemed to disturb him. "*What* would what me?" he asked cautiously.

I suddenly couldn't think of a thing. I didn't even know what what was, but I had to say something, so I said something: "Ill fares the land, to galloping fears a prey, where gobbledygook accumulates, and words decay."

I had just reached that Goldsmith paraphrase when a sub-researcher brought me the news from Washington that a movement is afoot in the nation's capital to cut down on bumblery, clarify officialese, and discourage certain platitudes (but not enough), in the wistful hope of bringing grace and meaning to the writing of English by government employees. I was glad to discover "finalize" among the banned gargoyles, but I don't see how the lawyers in Washington are going to get along without "predecease." The reformers, by the way, don't seem to know that this monster spawned an equally clumsy offspring, "survivorship." The main reason for this reform is to save filing space and money, but the economic aspect of the project does not depress me too much. It is a hopeful step in the direction of sense and sanity.

Come on, let's go out and get a breath of fresh air.

What's RIGHT with Science News Reporting?

Hillier Krieghbaum

When scientists gather at conventions, conferences, and informal "bull sessions," one recurring topic for discussion (and frequently, denunciation) is the role of the science news reporter of papers and magazines. Comments range from a plaintive, "Why did they phrase it that way?" to a vigorous, "Who wrote those damned headlines?"

At the outset of this discussion, let me admit that some reporting of science news (especially that by so-called "humorous" feature writers on metropolitan papers or the scientifically illiterate, small-town reporters) will get no defense at all from the skilled, professional science writers. Although it is no ground for justification or smugness, I might point out that the conduct of *all* lawyers, *all* physicians, or, for that matter, *all* research workers cannot be defended by their colleagues.

Most present-day science reporting rests on the premise that the people have a right to know what is going on. A part of the democratic creed is that an informed public is vital to sound public opinion and valid decisions. This is true in science, just as it is in politics, labor relations, business affairs, and other similar fields. Most scientists, I think, will agree with journalists on this.

More than 4 years ago, the AAAS Executive Committee said: "In our modern society it is absolutely essential that science—the results of science, the nature and importance of basic research, the methods of science, the spirit of science—be better understood by government officials, by businessmen, and indeed by all the people."

Beyond the supporting of democratic

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philosophy, there is another basis for reporting to the general public. Increasingly in recent decades, the public has directly supported science. Federal Government expenditures, raised by taxes on all the people, have zoomed to gigantic proportions. Direct contributions to many research organizations, particularly those concerned with medicine, have made possible multimillion-dollar expenditures. Most industrial grants have been made from profits piled up from popular acceptance of products or services.

Survey of Science News Reading Habits

Not only is there the people's right to know about scientific advances, but there is a craving on the part of large sections of the public for this information. Thanks to a pilot study of 200 persons recently completed by the Survey Research Center, University of Michigan, for the National Association of Science Writers and New York University, we now have some sound ground for estimating this interest in science news. The survey, made possible by a grant from the Rockefeller Foundation, showed the following.

1) More than three-quarters of the 200 persons interviewed—one-third of whom had attended college or far more than would be found in a typical United States cross section—read science news. The college group was expanded so that rough statistics could be obtained in the limited pilot study.

2) One-quarter of those interviewed read all the science items published in their local papers.

3) More than a third of the 200 persons wanted more science news printed. This point was further emphasized when those in this third of the sample were asked to name what types of news they were willing to curtail to make room for more science reporting. Some mentioned sports news; others cited society news; still others were willing to give up comics. The survey showed that a "sizable proportion" of even the occasional science news readers wanted expanded coverage. The SRC findings indicated "a potential for growth of the science audience at all levels of readership."

The pilot study for the NASW and New York University, limited as it was to only 200 individuals, still gave us a profile of those persons who are the most ardent readers of science news. For instance, the relationship between education and science reading was direct: the more education, the more science reading. Those who had taken either physical or biological science courses in high school or college, as a group, tended to read more science news than those who had taken no course or only general science.

Since educational background tied in roughly with probable occupation and income, it was not unexpected that these factors, too, correlated with science reading. Thus white collar workers and higher income groups were more interested in science news reports.

While he did not ignore the more practical applications of science in his choice of reading, a typical individual who read about science extensively and regularly tended to prefer stories concerning more abstract subjects. His interest, for example, was high in items about molecular theory, archeology, and space travel when he was given a list of possible news stories and asked to tell those that would interest him.

This group of high science readers, according to the pilot study, hold personal beliefs and values in accord with those of scientists, generally. They believe that the universe is orderly and understandable through scientific study. They evaluate science as a beneficial force and feel that it contains little threat to their well-being.

Scientists versus News Reporters

With this background about the most interested members of the public that science writers are serving, let us turn to some problems that arise when correspondents seek to report for this general audience.

Differences between scientists and news reporters arise, in a large degree, from differing points of view, differing philosophies toward the world in which they live. To the scientist, the new is something untried, a challenge, a matter on which one should be skeptical until it has been proved in the laboratory or field experiments. To the reporter, the new is news, something to be rushed into print—before a competitor gets it. If fuller explanations are needed, they may be told tomorrow.

The business of newspaper and magazine deadlines causes some ill will, at times with complete justification on the part of the scientists. A publication must be printed on an exacting time schedule, so that copies may be put on trains, buses, and trucks for delivery. These demands may be almost split second for the newspaperman, with 5 or 10 minutes making the difference between getting credit for a story or being "scooped." While scientists want to establish prior publication dates when there is rivalry, nothing in their activities compares with the reporters' battle against the clock. One news agency had a slogan, "Get it first but first get it right." To meet competition, some writers fail to carry out the second part of this instruction. Despairing of changing journalistic traditions, some reporters have speculated, in private, whether slower paced magazines may not be a more adequate medium for reporting contemporary science developments than the rapid fire of newspaper coverage. Personally, I feel that the professional science writer with broad background and many contacts still can do a satisfactory job. But he has eternally to keep in mind the caution, ". . . but first get it right."

The science news reporter thinks of himself as a representative of the public and, as such, the better ones seek to find out the facts and to present them in their proper perspective. In this process, these correspondents may step on protruding toes and the anguished scientists may blame not their own deficiencies but the inquisitiveness of the writers. In these cases, both newsmen and scientists are interested parties and may fail to maintain the objectivity that is a hallmark of true scientific research.

The most common charge against the contemporary science reporter is that he sensationalizes. This complaint headed the list in the recent 200-person survey, as it did an earlier NASW–New York University attitude study of a random sample of scientists listed in *American Men of Science*.

This charge of sensationalism arises because the science news reporter must translate from the technical language of the laboratory report or theory into the colloquialisms of the man in the street. If a writer follows too exactly the technical or complex descriptions of the scientist, he fails to communicate to the typical reader who may never have taken high-school physics or chemistry courses. If he popularizes too extensively, he fails to retain the full flavor of the research, presenting only a distortion of the real results after the manner of amusement park mirrors.

Help from the Scientists

Many scientists recognize the difficulties of the reporters' translating job. For instance, Fritz Lipmann in his speech at the dinner for distribution of the 1953 Nobel prizes in Stockholm referred to those workers whose "findings mostly have to be expressed in a scientific language which is understood by only few."

Austin H. Clark, who went out of his way to work with reporters during his long career with the U.S. National Museum, once illustrated how translating into popular language could be done without mangling science. He confided to science writers that he thought a scientific paper in which he said, "Most cuckoos, the honey-guides of Africa, the

weaver finches, some hang-nests, our cow birds, the rice-grackle, a South American duck, and, according to recent information, one of the paradise birds, lay their eggs in nests of other birds which hatch these eggs and raise their young," could be translated without valid objection into this lead paragraph: "Those unfeeling mothers who leave their babies on the doorsteps of prosperous people's houses have their counterparts among the birds."

Commenting on this topic, one man, a college graduate, interviewed in the 200-person sample of the recent survey, said: "Consider us all pretty ignorant and give us ABC's. Then give us a little more at a time—groundwork first, with subsequent enlarging on fundamentals."

Of course, there is no defense for falsification by a science writer, but there might be more sympathetic understanding of the reporters' demanding assignments. Scientists may help in this translating job if they will work with reporters when time permits.

Back in the 1930's when I was writing science news for the United Press, I was fortunate enough to attend a press conference arranged before Robert A. Millikan presented a highly technical paper to the National Academy of Sciences. Millikan took more than an hour to explain in simple language what he was going to say the next day and to answer writers' questions. One reporter asked the Nobel prize winner if his work could be described in a particular way—translating the complex findings into terms of space travel. Millikan said the work could be so described. The following day that reporter's lead or first paragraph was a paraphrase of the version accepted by Millikan. This was science reporting of high caliber.

Sometimes reporters do not have time before their deadlines to check with scientists. Other times the scientists are unavailable or unable to translate adequately, even when they are asked to do it. Human weaknesses may contribute to a sad performance, and it may be due to either writer or scientist.

Is Science News Reporting Adequate?

In the recent survey of 200 representatives of the general reading public, more than half were satisfied with the present-day presentation of science news. Of those who offered suggestions for improvements, 30 percent wanted simpler language and more lucid explanations; 20 percent desired more details and greater completeness.

In an earlier study by NASW and New York University of a random sampling of persons listed in *American Men of Science*, approximately a third of the 113 who replied thought that contemporary science reporting was adequate, and nearly a third more found it reasonably good with specific exceptions, such as small-town home newspapers and occasional inaccurate news articles.

If these two surveys are correct, then the oft-flaunted charge of sensationalism on the part of science writers seems to break down for the professionals now covering the field. Infrequently slips do occur, but apparently they are not common.

Headlines are another sore point in scientists' discussions of press coverage of their activities. Two technical points should be understood by these critics. First, in all but the smallest newspaper offices, the reporter does not write the headlines for his stories. A specialist does this. The headline writer or copyreader, as he is called, composes the headline upon what the reporter has written in his story. If vagueness and ambiguities are included, the copyreader may misinterpret facts when he writes a heading. This system may be a weakness, but it is the way contemporary publications are put together. Second, a headline is an exceedingly complicated device; there can be only so many letters to a line of type. If the news concerned, for instance, sulfanilamide and para-aminobenzoic acid, the headline writer obviously would be unable to use these terms in a two-part heading, each line of which could total no more than 18 letters.

Some scientists express disappointment with newspaper and magazine articles that do not permit them to reproduce the experiments being described in popular publications. Such news articles do not seek to give full details. It is unfair, I believe, to criticize the mass circulation journals for not providing what only the specialists want. Let the scientists turn to their own publications for this information. And when public interest is so great that a popular version is flashed to all sections of the country, let electronics and other media of mid-20th century speed the details to the specialized audience.

Is part of the difficulty here caused by a conflict between scientific accuracy and newspaper accuracy? I think so. To me, scientific accuracy supplies all the details necessary to explain the project or to reproduce the experiment. Journalistic accuracy, on the other hand, is simply an attempt to convey an accurate impression or a correct picture of what this scientific development means to the general public.

When Albert Einstein propounded his theory of relativity, it was frequently said that only a few hundred persons in the world really comprehended his work. But the correspondents did give the public a generally accurate picture of the implications of this theory. Thus Einstein's philosophic contribution to modern thinking became a part of the contemporary scene while his abstractions still elude most people. If present-day science reporters are doing equally well for advances of science today, then I think they are discharging their basic responsibilities in a democratic society.

Our debt to tradition through reading and conversation is so massive, our protest or private addition so rare and insignificant—and this commonly on the ground of other reading or hearing—that in a large sense, one would say there is no pure originality. All minds quote.—RALPH WALDO EMERSON.

The Educated Man in 1984

J. Bronowski

When we ask what today's education should do, the man we have it in mind to inform and to form is the man of 20 and 30 years hence. And for reasons that will become plain, the year 1984 carries a symbolism that our education ought particularly to keep in mind.

I

Education in schools and universities has many functions. I shall confine myself to two, both of them dominated by the skills that we need in order later to carry on adult life. Education as I shall discuss it is therefore learning to do something quite precise; and I shall not discuss those other general values that the child and the youth must also draw from his life at school and university.

I have said that the part of education that occupies my mind is the learning of something quite precise. Even so, however, there are different things to be learned—or better, there are different purposes for which we learn things. I want to distinguish between two purposes, which differ one from the other in their specificity.

The young man at night school learns bookkeeping in order to keep books. An engineering student learns the calculus in order to become an engineer. A historian learns Medieval Latin in order to read documents. I learned Italian in order to read papers in mathematics. These are examples of education for a very specific purpose, and since this purpose often helps us to earn our living, I think of this as vocational education.

But I knew a man once (he was a schoolmaster who had just retired from teaching mathematics) who learned Italian in order to read Dante. You will see that what he learned was indeed precise, and the purpose for which he learned it was specific. And yet I cannot feel that this was vocational education. The learner was not fitting himself for a task, as if he had been a literary critic by profession. He was fitting himself to derive from the work of Dante a larger, a deeper sense of the many-sidedness of human life than had reached and stirred him in translation. He was fitting himself, even at the age of 65, not to make a living but to live, and to take not merely his place but his share in human society.

This is the purpose for which we learn English and arithmetic and history at school. For these are subjects that do not stop at a single need, at writing letters and at making out bills. To read and write, to reckon, to remember the French Revolution, these are actions that penetrate all through the branching nexus, which underlie the whole fabric of our society. Everything in these skills is precise, but it is not specific to one purpose; instead, it is part of a hundred arts and a thousand livelihoods. You cannot translate from the Russian unless you learn Russian, but you cannot contribute anything at all to English society unless you learn English. This is the purpose of an education in such subjects as English and arithmetic and history, without which no vocation is accessible to you. These subjects inform and hold together the fragments of society, so that they form and in a sense are its culture.

The subject of this article is science as part of culture, and a scientific education as a necessary part of our cultural education. This is a broad subject and I do not want to gloss over that. I do not want to pretend that it is something else; for example, that it is the more fashionable complaint that scientists themselves are uncultured. Yes, most scientists are uncultured; and so are most surgeons and administrators and Greek scholars and boards of directors and doctors of divinity; so, in short, are most men who are too busy to write letters to *The Times* in their own defense. I want more scientists to be cultured, literate, and human: I hope that I have shown that on other occasions. But I am not in the least impressed when I am lectured on the subject by bishops in the correspondence columns of newspapers, or by generals at school prize-days. When the pundits advise scientists to learn something more, they make it too plain that they find it easier to give good advice than to take it themselves.

Our society is indeed divided between the past and the future, and it will not reach a balanced and unified culture until the specialists in one field learn to share their language with those in another. The scientist has much to learn still, in language and thought, from the humane arts. But the scientist also has a share, a growing share, to contribute to culture, and humanism is doomed if it does not learn the living language and the springing thought of science. The bishops and the generals have some learning to do too: an enormous piece of learning if what they and I value is to survive, and society to become one. My subject is science as part of culture, and a scientific education as part of our cultural education, because for lack of these life-giving parts the ancient cultures are dying at the roots.

II

Here I am often stopped by those whose education and tastes are literary, because they find these claims puzzling. They know what culture is: it is Sophocles and Chaucer and Michelangelo and Mozart and the other figures round the base of the Albert Memorial. And they know what culture is not: it not laundry lists and sleeping-pills and the proved reserves of oil and the *Statistical Digest*. In short, culture is not a body of facts: but what is science but facts? How then, they ask, can science be a part of culture, and why should one learn science to become cultured? There is no scientist in the frieze of the Albert Memorial.

I have corrected this mistaken view of science a number of times, and particularly in my Carnegie lectures to the Massachusetts Institute of Technology. Therefore I will not stop to dispute it now except to say that it misses the very meaning of science; for science is not a set of facts but is a way of giving order, and therefore of giving unity and intelligibility, to the facts of nature.

But I do not need to discuss this large question at present, because the history of education shows that, whatever culture itself may be, the means by which we must teach it are always precise: as precise as my old schoolmaster learning Italian. When the Society for Establishment and Support of Sunday Schools was founded in 1785; it opened the path by which its teachers were to take the children into the Bible with the words: "Be diligent in teaching the children to read well." About the same time, the great dissenting academies (speaking through Joseph Priestley) held that all education must have for its end the pursuit of truth and the practice of virtue;

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and they taught both, as Oxford and Cambridge did not teach them, in a factual curriculum of logic and medicine and modern languages and mathematics and some science. Against this range of subjects, Lord Eldon long defended the culture of the old grammar schools; for example, in the famous case of the Leeds Grammar School in 1805; but even that dying culture was built, as Lord Eldon's judgment insisted, on two precise subjects, Latin and Greek. The founding of the University of London on the initiative taken by the poet Thomas Campbell in 1825, the changes at Oxford and Cambridge about the same time, the rise of the Red Brick Universities later in the century—all these are signs of a profound buckling and shifting of culture. But they are not vague signs, and the changes they made are not imponderable. They are precise changes in what was taught.

The fact is that the syllabus of schools and universities is always in movement from the first of the two educational modes that I am examining here to the second: from vocation to culture. No doubt reading was first taught at Dame Schools as a strictly useful skill; certainly Latin and Greek were first taught at grammar schools for clerical use; and arithmetic and technical drawing and debating were taught in the Mechanics' Institutes in order to help those who learned them to make their way in the world. I will remind you that the show of industrial drawings which the Society for the Encouragement of Arts, Manufactures, and Commerce held is one of the beginnings of the Royal Academy.

This example makes my point trenchantly. Some subjects in the syllabus remain vocational subjects; indeed, even their vocational use may shrink in time until, like spherical trigonometry and graphical statics, they shrink out of the syllabus. Other subjects turn out to have a wider range of uses; men find that, whether they are farmers or mechanics or bank clerks, they cannot do without them; and, above all, that they cannot do without them as members of society, whatever their profession. So in time these subjects cease to be the prerogative and the burden of specialists, and become general needs. English and arithmetic and history and now French have moved in this way, from the special to the universal, from the vocational subject to a place in our culture. This is the sense in which I have defined a cultural subject. And it is this sense, this movement, that is now patent in the growth of science in our society.

III

Science was once the concern of specialists, and now enters into the life of everyone. The switchboard and the motor car, the treatment of flour and of cigarette paper, the building of a crèche and of an atomic pile, are our daily concerns; by these we move and act and live. We simply cannot dissociate ourselves from the hot-water system and the air-mail and frozen food and the Linotype machine. A nation unskilled in these, a nation in which the screwdriver and the fuse-box are still handled with suspicion, is today a backward nation.

And this goes deeper than the mechanics of earning our living. When a society is penetrated, as ours is, by technical skills and engines, the decisions of state cannot be taken out of the context of science. You cannot as a voter advocate a policy on myxomatosis with any responsibility unless you have a general sense both of the ecology and of the economics of rabbits. You cannot ponder the decisions that we have to make on the development of atomic energy without some understanding, among other things, of how human inheritance works; you have no right to talk about war and peace, and to vote for rearmament or disarmament, without that. And no member of Parliament and no minister can make intelligent judgments on that most profound of contemporary issues, the secrecy that surrounds fundamental atomic research, until he is at home in the tradition of science since Giordano Bruno and Galileo.

The fate of a nation may hang on an error of judgment here. Let me give you a slightly mischievous example. In 1945, the British Government published (as a parallel to the American Government's Smyth Report) a White Paper on the wartime development of atomic energy. Among the documents in this White Paper is the directive by which Mr. Winston Churchill, as he then was, set up the project to make an atomic bomb. This directive begins with the words: "Although personally I am quite content with the existing explosives. . . ."

This bland phrase is a monument to a nonscientific education. Think what it would have implied in a dictatorship—in which, as the example of Germany shows, the dictator is surrounded by specialist advisers who are yes-men, and who are therefore bigoted and ignorant even in their specialty. In a dictatorship, Mr. Churchill's satisfaction with existing explosives would have been the end, not the beginning, of serious research toward an atomic bomb. The great man says that he personally is content with the existing state of science; who then would be strong enough to show discontent? I do not much care for atomic bombs myself, but still less do I care to have them judged in phrases like Mr. Churchill's. In 1941, they might have weighed life and death between this country and Germany; and what brought down the scales was not the wisdom of statesmen, but the democratic tradition which caused Mr. Churchill to waive his own unwisdom.

This example shows us succinctly what voters and statesmen do not know. I have called Mr. Churchill's astonishing phrase a monument to a nonscientific education. For it could have been written only by a man, an intelligent man, who simply does not understand how big a million is. The difference between atomic explosives and ordinary explosives is the difference between the length of a nuclear bond and a molecular bond; and this is a factor of more than a million. To suppose somehow that, in multiplying the energy of an explosive by a million, you are doing nothing very different from multiplying it by 2, or 5, or 10—this is simply not to grasp the scale of the world.

And the public does not grasp it. To say "ten to the sixth" to anybody today, however educated, is still to invite the reproof that one is stressing mere numerical detail. The nonscientist lacks such conceptions, and their lack cripples his judgment in the modern world.

Here we reach the nub of what we mean by a culture. Of course, we do not want members of Parliament to be atomic physicists or experts in virus diseases; I do not even want them to be mathematicians. Why should they be? They are not literary critics or historians. Yet, without being specialists, they know the difference between Milton and Kipling, and what sentiments each of these minds stands for. They know that Pitt and Napoleon were contemporaries and that, in the nature of things, the Industrial Revolution in England came before and not after the American Civil War. But in the field of science, the voters and those whom they elect have absorbed no such implicit knowledge. They have no framework into which to fit new information, no standards to test it by, and no vocabulary with which to handle it. If I were to say with enough solemnity that the stars must be very young because they are made of neurons and enzymes, no statesman would wink at me. Indeed, in Nazi Germany Himmler planned to found an institution to prove that the stars are made of ice.

IV

To make science familiar as a language, we must start in the schools. In England, a beginning has been made in the general science course in grammar and public schools. Yet this course still leans too far toward vocational detail, and is not bold enough in the belief that the concepts and principles of science are part of our culture. General science suffers, of course, from two handicaps: it has to be planned as part of the education of the scientist as well as of the nonscientist, and it has to be tied to the moderate and rather dull resources of

school laboratories. I accept these handicaps, but, even so, I want to fix our minds much more firmly on the lively nonscientist. What can I propose for him?

First, I want to propose less mathematics than he studies now, but of a more practical kind. That is, I want to change the stress from mathematical manipulation to the meaning of the sales tax and the twinning rate. The language of mathematics is still taught as a dead language, nearly all grammar. I want instead to teach more translation, from the everyday facts into mathematics and thence back into the everyday.

As one corollary, I believe that we need to make statistical methods part of the education of everyone in schools and universities: and this for two reasons. One is that only from statistics can the nonscientist learn to use averages and approximations with confidence, to be unafraid of dividing the national debt or the German battle losses by the population, and to know in his bones the difference between a million and ten. The other is that modern mathematical statistics is a new view of science, which I believe will transform it and will replace the mechanisms of Newton by the more subtle concepts of modern physics.

There is another way in which I want to see the teaching of mathematics changed. I think we should be less preoccupied with number and quantity and more with relations of order and arrangement. This vision of the world by shape and structure is, I think, characteristic of the newer sciences, and the mathematics teacher can help to make it familiar.

I am thus brought to my second group of proposals, which concern physics and chemistry. Here I think that we should make the atomic picture central to the teaching of both subjects much earlier than we do. There is no need to wait until the chemistry student gets to organic compounds, or the physics student to crystal structure. These pictures of the way atoms are assembled can be made real and exciting to the nonscientist, whose visual sense is often much stronger than his gift for handling either concepts or symbols.

And I think the nonscientist can in the same way be inspired by the outlook of statistical mechanics, much more than by doing Boyle's law with a piece of barometer tubing. This is really the strength of the atomic model for the nonscientist, that it lends itself both to geometrical and to statistical thinking; and I should like to see it fully used for both.

My third plea is for more biological teaching than we have now. There is a wealth of natural material and natural interest in plants and insects and the animal processes, which somehow we allow to wither in the grammar school. And as in the other things that I have underlined, it has both a geometrical side, for example, in plant structure, and a statistical side, for example, in animal genetics.

But more than this, I have the sense that biology offers us a path into scientific thinking by way of the pleasures of collecting and of skill of hand. Liking to collect, to observe, to draw, to take apart and to put together—this is how many children and adults of little other education find their way into the arts. And I feel that there is a way through these also into the sciences: a way that is valuable exactly because it makes art and science one, as Leonardo da Vinci and Andreas Vesalius did.

Fourth, I think we need to teach science, even at school, not as a collection but as an evolution of knowledge. I think this important for three reasons. Because it sees science as a historical development, it offers links with history and literature and geography that can give help and a vivid perspective to the nonscientist. Because it presents science as changing, questioning, and argumentative, it can teach the methods of rational debate to everyone in the classroom, and this can be a lifelong lesson. But most important, the evolution of science goes to the heart of the scientific method: for it shows at each step how the logical deduction from what seems to lie behind the known facts must be confronted with experience. We make an induction, we put the deductions from it to test, and on the results of the test we base a new induction. This to and fro between the logical and the empirical is the core of the scientific method, which nonscientists never seize because they do not see science as a progress.

To these four general points I will add a fifth. It is that every boy and girl, every undergraduate, should do one small piece of personal scientific research. It need not be a pompous project, with much search among reference books and writing to distinguished strangers. It can be as simple as the pitch of an insect's wing-beat, or the composition of a rock, or growing a crystal. What is needed is that it should take a little time, a little reading, and more thinking, and that it should be done alone. I believe that this small practical experience could change the light in which nonscientists see the long and unsung vigils of the solitary research worker.

V

I do not want at the moment to open the practical questions that these proposals raise. For example, if we put what I suggest into the syllabus, then we shall have to take something out. What is it to be? What can we sacrifice in the rich distillate of human knowledge—and in the ancient and forbidding history of human error? I have, of course, thought about this, and I hope at another time to discuss it; but now is not the time. For if I were to discuss now the merits of rival subjects in the syllabus, we should at once lose the sense of the urgency of what I propose.

And it is urgent. Science must become as a subject part of our culture, or we shall fail, not to train scientists, but to preserve our culture. This is the danger that the letters to *The Times* and the prize-day speeches miss. They speak as if we need do no more than give a little grace and dignity to a few uncouth scientists. But the scientists are inheriting, they are conquering the earth, and if you do not speak their uncouth language, then you will sink to the status of the native yokels when the Normans overran England.

I have called my article "The educated man in 1984" in order to warn of this danger. It is certain that the educated man in 1984 will speak the language of science. This is not at issue. The issue is something else. Will the educated man in 1984 be a specialist, a scientist or technician with no other interests, who will run his fellowmen by the mean and brutal processes of efficiency of George Orwell's book? Or will he be a statesman, an administrator, a humanist who is at home in the methods of science, but who does not regard them as mere tools to efficiency? The choice between 1984 and an earthly paradise does not depend on the scientists but on the people for whom they work. And we are all the people for whom science works.

H. G. Wells used to write stories in which tall, elegant engineers administered with perfect justice a society in which other people had nothing to do except to be happy: the Houyhnhnms administering the Yahoos. Wells used to think this a very fine world: but it was only 1984 or Aldous Huxley's *Brave New World*. A world run by specialists for the ignorant is, and will be, a slave world. A man of taste who sneers at machines, a housemaster with his eye on the preponderance of university scholarships in classics, a civil servant who still affects to despise science, is abdicating his share of the future and walking with open eyes toward slavery. By leaving science to be the vocation of specialists, they are betraying democracy so that it must shrink to what it became in the decline of Athens, when a minority of educated men (who had to be paid to make a quorum) governed 300,000 slaves. There is only one way to head off such disaster, and that is to make the educated man universal in 1984. This is the force of my argument here, to make the language of science part of the education, the cultural education, of the young who will have either to make or to suffer 1984.

English Style in Scientific Papers

John R. Baker

Many scientific papers published in Great Britain are written in a style quite different from that adopted by good English authors. There would be no disadvantage in that, if an improved version of our language had been invented for scientific papers. This, however, is not so. For clarity and directness one must turn to nonscientific authors. One hesitates to direct attention to this subject lest one be accused of setting oneself up as a stylist. There is no such intention here. It is proposed merely to suggest a few ways in which scientific papers could be made simpler, clearer, and more pleasant to read.

To prevent any misunderstanding, it must be remarked at the outset that it is no part of my purpose to try to oppose the use of necessary scientific words. On the contrary, the precise use of certain technical terms, carefully defined and internationally understood, is an obvious necessity. We shall be concerned here with the words that stand between the technical terms.

If one examines all those passages in scientific papers that are least in accord with good English style, one finds that there are three main kinds of error, which may be described briefly as those of grammar, grandiloquence, and German construction.

It would be absurd to treat the subject of grammar at any length here, since good advice on this subject is so readily available. Grammatical errors in scientific papers sometimes make it impossible to be sure what a sentence is intended to mean. In other cases the mistakes are troublesome only because they distract the reader's attention from the subject under discussion. The words ending in *-ing* (especially *using*) are very frequently misused. One repeatedly finds such statements as this: "Examining a capillary under the microscope, after staining with carmine, it resembles a homogeneous hyaline tube." Yet anyone who is intelligent enough to carry out scientific research at a university can easily grasp in 15 minutes everything that it is essential to know about the use of present participles and gerunds.

The menace of grandiloquence seems to derive from the time when doctors used many Latin words; and indeed these errors are much more common in medical and biological than in chemical and physical writings. It is difficult to account for the fact that an Englishman will write *"vide supra"* when he means "see above." It is noticeable that those who use Latin tags most freely do not always know enough of that language to realize that the word *data* is plural.

Long words derived from Greek or Latin roots are often used for expressing very simple ideas. When a man wants to say that something is visible to the naked eye on merely opening the body-cavity, he tries to make it sound grand by saying, "this phenomenon can be macroscopically observed upon laparotomy." Sometimes he uses short words, but chooses an elaborate way of expressing himself. Thus, if he wants to refer to a sheet of metal about 1/100 in. thick, he says that it is "of thickness $\sim 10^{-2}$ in." If his intention is to tell his readers that the longest of the six pairs of chromosomes in a cell is about three times as long as the shortest, he writes, "the maximum size range of the six pairs as represented by the ratio length of chromosomes 1/length of chromosome 6 is about 3/1."

The grandiloquent writer brings in abstract words where none is required. Instead of writing "because the surface of the retina is spherical," he substitutes "because of the sphericity of the retinal surface." Instead of saying that something is near the nucleus, he says that it "occupies a juxta-nuclear position."

Genteelism is allied to grandiloquence. People who think it polite to call a napkin a "serviette" have their counterparts in science, who cannot soil their mouths with paste or mash but must say "Brei" instead. Another example is "sacrificed" for "killed." Allied to genteelism is the deliberate use—not just occasionally, but over and over again—of archaic words instead of their exact equivalents in plain modern English (for example, "save" for "except").

Vogue-words have their place in the vocabulary of the grandiloquent; but what may be called negative vogue-words must also be mentioned here—perfectly good words that for an inexplicable reason must not be used. Why must the word *about* never be used by the grandiloquent? Why must they always say *"circa," "ca.," "c.,"* "approximately," "around," "of the order of," and even "~"? What is the objection to "separate"? Why must "discrete" invariably be substituted? (A case can be made for the occasional use of "discrete," when special emphasis on complete separation is necessary.) *After* is another negative vogue-word.

Grandiloquence and related foibles, however, are not the worst enemies of good English. A still more serious and more insidious evil exists.

In ordinary speech writing it would never enter anyone's head to say "a tea containing cup"; one would naturally say "a cup containing tea." Yet in scientific journals one will find (for example) "iron containing globules," when what is meant is "globules containing iron." It is questionable whether the writers of such phrases have ever asked themselves why they adopt this construction, which is strange to our language and does nothing to improve it. Why do they say "a hyaluronidase treated area" when they mean "an area treated with hyaluronidase"? Why place a complex adjectival phrase before the noun, and why omit the clarifying preposition? Why say "nonformalin containing fixatives" instead of "fixatives not containing formalin"? Why "eight micra thick sections" when one means "sections 8 μ thick"? Or why does a man who would never dream of saying "the Jones associated people" write of "the nucleolus associated chromatin"?

These, however, are relatively innocuous examples, compared with many that disfigure our scientific journals. "A methylene blue stained preparation of rabbit ear skin" is bad enough, but not quite so bad as *"Dytiscus* red flight muscle sections" or "the use of acidified (by HNO_3) or alkalized (by ammonia) solutions of silver." Even these, however, are not the worst English that it is possible to write, if one is determined to pile up qualifying words other than adjectives in front of the noun they qualify. The limit is perhaps reached by "adenosine triphosphate activated actomyosin contraction." (To spare the reader trouble, I may mention that this means the contraction of actomyosin, activated by adenosine triphosphate.) These are words put together without the slightest attempt at clarity or any consideration

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for the reader. The phrase is worse than merely illiterate: it is rude.

This piling-up before a noun of words that are not adjectives, but are used adjectivally, is rather a new fashion in English scientific writing. It was seldom seen in Britain a quarter of a century ago. There can be scarcely any doubt about its origin. That many American scientists are of German descent is obvious when one looks at lists of authors in journals published in the United States. The process of piling up adjectival phrases is familiar to everyone who knows the German language. It does not make for clarity. It comes naturally to a German to write "die einzigen, durch unmittelbare Beobachtung sicher zu ermittelnden Stellen" or "das von dem Objectiv-system in der der Lichtquelle zugeordneten Ebene entworfene Beugungsspectrum." Unnecessarily difficult though this construction may seem to an Englishman, yet the German language does at least help us, through the inflection of the words, to follow our path through such passages as these. But when this kind of construction is forced artificially on our much less inflected tongue, and the prepositions are omitted, all pretence of clarity is lost.

The German-American style did not originate with men who had set themselves the high ideal of improving our language as a vehicle for scientific ideas. It was, on the contrary, the product of childhood, introduced by people whose parents had spoken in sentences constructed in their native, German way. It is strange indeed that an Englishman or Scot, accustomed since infancy to a simple syntax, with logical order of words, should copy a German-American imitation of English just because he happens to be writing in a scientific journal. Our own language, as written by those who know and love it, can scarcely be surpassed for clarity, directness, and simplicity.

It is noticeable that scientific books published in the United States are often written in a style that is close to ordinary English (though the American people have their own special words, spellings, and phrases). The difference in style between American books on one hand and papers on the other suggests that the publishers correct the style of writing where necessary while the editors of journals do not. It must be remarked that the German influence is altogether lacking from some American scientific papers which are models of lucidity and good style. A paper by Michaelis (*1*) on the reaction of dyes with nucleic acids may be quoted as a particularly fine example.

How can the standard of English in scientific journals be improved? One thinks at once of the obvious works of reference—of Fowler (*2*), Partridge (*3*), and Gowers (*4*). Quiller-Couch's lecture "On jargon" (*5*) is packed with good advice and horrid examples. The teachers of English in our schools could help very much, if they were to study the errors made in scientific papers and base their instruction to science students on their findings. Style, however, is largely dependent on example. Good reading makes good writing. One could almost imagine that some of the contributors to our scientific journals had never read anything but German-American. Yet good examples abound, in books of widely different scope. A trio so diverse as Macaulay, P. G. Wodehouse, and Sir Winston Churchill have this in common, that their English is lucid. There is no dearth of good examples to suit every taste.

A strange fact that gives some basis for optimism is this. When an author has finished the scientific part of his paper, he often addresses a note of general information to the reader. At this point he suddenly discloses for the first time that he can write English, for his ideas are clearly expressed. If one wished to translate what he now says into the style adopted in the rest of the paper, one would have to write something like this: "Some related interest possessing observations by the present writer *et al.* will be the subject of *ca.* $10^{0\text{-}3010}$ discrete communications" (*6*). The fact that he does not write like that shows that he need not have done so in the body of the paper.

One last, necessary word. The best English writers occasionally use some of the strange constructions mentioned in this article, often to produce a special effect for a particular occasion. They do so, however, only at long intervals. The greater part of their writing is so smooth and fluent that the reader forgets that he is reading and knows only that he is absorbing ideas.

References and Notes

1. L. Michaelis, *Cold Spring Harb. Symp. Quant. Biol.* 12, 131 (1947).
2. H. W. Fowler, *A Dictionary of Modern English Usage* (Clarendon, Oxford, 1937).
3. E. Partridge, *Usage and Abusage: a Guide to Good English* (Hamilton, London, 1948).
4. E. Gowers, *ABC of Plain Words* (H. M. Stationery Office, London, 1951).
5. A. Quiller-Couch, *On the Art of Writing* (Cambridge Univ. Press, 1925).

6. With the exception of the passage in quotation marks in this paragraph, all the examples given in this article are genuine: none was invented by myself or modified to suit my purpose. I am indebted to A. C. Hardy for a careful criticism of the first draft of this article.

Suggestions for Contributors to *Science*

To assure publication of papers with a minimum of delay and to provide readers with subject matter of a wide range and general interest, the AAAS editorial board has established the following policies.

Papers submitted by members and nonmembers of the AAAS will receive equal consideration for publication. Material that is not considered suitable for publication by the editorial board will be returned to the author, and it cannot be the subject of continuing correspondence.

Types of Articles

1) *Lead articles.* Lead articles varying in length from about 1000 to 10,000 words are invited for consideration. These may be general articles or reviews of recent advances in some field. Authors should keep in mind the broad audience of *Science* and should employ reasonably nontechnical language that will be intelligible to most readers.

2) *Reports and Letters.* All articles commonly called technical papers and communications, as well as letter-to-the-editor type of comments, are now placed in one department. Reports of research should be limited to four double-spaced, typewritten pages (about 1200 words). This includes the space occupied by figure or table, references, and author's name and affiliation. However, statements of conclusions without supporting data will not be accepted. *Such data should be included to the extent necessary.* Illustrative material should be limited to one table *or* one figure.

Brief announcements of completed work or observations varying in length

from several sentences to approximately 600 words will usually be given priority for publication. These may be reports of significant research that will be described in detail in an archive journal. Supporting data or a comprehensive account of the work should be included in the letter of transmittal for the guidance of the referees.

Letters to the editor are usually comments on previously published articles.

Every effort will be made to publish papers promptly. This will require the cooperation of authors, who should follow these suggestions: (i) give special attention to concise expressions; (ii) supply clear figures that unquestionably are suitable for reproduction; (iii) see that manuscripts are submitted in the form and style used by *Science*, especially with regard to references, figure legends, table headings, and abbreviations; (iv) on the galley proof, limit alterations to typographic and factual errors; (v) return galley proof promptly.

Every article, communication, or book review is accepted with the understanding that it has not been published or accepted for publication elsewhere. (Occasionally an important article is reprinted from, or simultaneously printed with, another periodical, usually one not readily available to readers of *Science*, but this is always done by special arrangement with the author.)

An article should receive a thorough review before submission, if possible by someone other than the author. If a manuscript is returned to an author for a thorough revision, the revision date will be regarded as the "Received" date for the article.

All manuscripts are to be submitted in duplicate, in order to expedite reviewing by referees and editorial processing. In the event that a paper is returned to an author for revision, one copy will be retained in the editorial office pending final disposition.

Preparation of Manuscript

For the first copy of the typescript, use a good grade of bond paper, 8.5 by 11 inches. *All copy, including quotations, footnotes, tables, literature references, and legends for figures, should be double-spaced.* Leave margins of at least 1.5 inches at the sides and at the top and bottom. Pages that are heavily corrected should be retyped. Do not insert corrections and additions lengthwise in the margin; the printer works with the typescript mounted in a frame that exposes only a few lines at a time.

Changes in copy and proof. All corrections in manuscript *and* proof should be transmitted to the editorial office in Washington, *not* to the printer.

Illustrations. A brief legend should be provided for each diagram, graph, map, and photograph. It should *not* be incorporated in the figure. All legends are set in type by the printer and, hence, should be typed double-spaced, on a separate sheet of paper.

On the margin or back of each illustration, write in pencil the number of the figure, name of the author, and abbreviated title of the article. All illustrations should be packed carefully with cardboard to avoid damage in mailing. Cracks and marks made by paper clips or pressure of writing ruin photographs for reproduction.

Line drawings should be made with India ink on heavy white drawing paper or blue tracing cloth. Ruled coordinate paper may be used for graphs, provided that it is printed in light blue; the important coordinate lines and scale markers that are intended to appear in the reproduction must be ruled in India ink.

A good size for a drawing is twice that desired for the printed figure, with all lettering and line thicknesses similarly enlarged. One-column-width illustrations are reproduced 2⅛ inches wide; two-column width illustrations, 4-9/16 inches wide; three-column width illustrations, 6-15/16 inches wide.

Diagrams containing little detail should be planned so that the printed figure can be made one column wide. For presenting apparatus, a line drawing is usually better than a photograph.

Photographs should have a glossy finish. For satisfactory reproduction, a print must be unblurred and must show sharp contrast between light and dark areas.

When it is desirable to indicate the magnification in photomicrographs, the scale in microns (or other suitable units) should be drawn directly on the print rather than indicated in numerals in the legend. This allows more flexibility in scaling for reduction and, hence, greater accuracy in indicating size of the subject.

Tables. Each table should be typed on a separate sheet and should be provided with a title. Tables should be numbered consecutively with Arabic numerals.

References and notes. The only footnotes are to be those appended to tables and the author's affiliation in lead articles. All other explanatory notes, including acknowledgments and authorization for publication, and literature references are to be numbered consecutively and placed at the end of the article, under the heading "References and Notes."

Date of publication. Although an author will not be notified of the particular issue in which his paper will appear, receipt of galley proof is a fair indication that publication is imminent—usually within 3 weeks. If such a schedule is to be maintained, it is essential that the author mail his corrected galley proof to the editorial office (1515 Massachusetts Ave., NW, Washington 5, D.C.) within 24 hours after receiving it, and that he refrain from making extensive alterations.

Reprints. Reprint orders are handled by the printer, Business Press, Inc., Lancaster, Pennsylvania. An order form will be sent to the author along with the galley proof. If reprints are desired, this form should be filled out and promptly returned to Business Press. Any subsequent correspondence *concerning reprints* should also be addressed directly to Business Press. In the case of authors living outside the U.S.A., the printer requires payment in advance for reprints and postage.

Preparation of Graphs

These suggestions do not comprise a comprehensive set of suggestions but merely emphasize certain practices that often are disregarded. Most of the recommendations are in accord with those made in American Standards Association publication Z15.3.

Not more than three or four *curves* ordinarily should be shown on the same graph, although more may be included in the case of a family of well-separated curves. Use a solid line for an especially important curve and dashed, dotted, or lighter solid lines for the other curves. No curve or coordinate ruling of the graph should run through any lettering or outlined circles, triangles, and so forth, that are used to indicate plotted points.

Coordinate rulings should be limited in number to those needed to guide the eye in making a reading to the desired degree of approximation. Short scale markers, or "ticks," may be inserted between rulings if this is desirable. The rulings should be light enough not to distract attention from the curves being presented.

Lettering should be placed so as to be easily read from the bottom and from the right-hand side of the graph; that is, the lettering should face either the bottom or the right-hand side of the drawing.

Explanatory comments, supplementary data, or formulas should be placed in the figure legend or in the text. The exception to this rule is the case where there are several curves on the same graph that need separate identification; if practicable, they should be identified by brief labels placed close to the curve (horizontally or along the curve) rather than by single letters or numbers requiring a key.

If it seems necessary to place supplementary information on the drawing proper, the lettering should be kept within the vertical and horizontal limits of the curves or other essential features of the drawing. Otherwise the space occupied by the drawing may be needlessly

large, or else the drawing may have to be reduced in reproduction, often to the point where the lettering or other details are illegible.

Scale captions should be placed outside the grid area, usually at the bottom toward the right for the horizontal scale and at the left-hand side toward the top for the vertical scale. The scale caption should consist of (i) the name of the variable plotted, (ii) its symbol, if one is used in the text, and (iii) in parentheses, the abbreviation for the unit of measure; thus, Pressure p (lb/in.^{2}). Avoid using such captions as "Pressure in lb/in.^{2}" and "Pressure in lb per sq. in." The technical terms, symbols, and abbreviations on a drawing should be in accord with those used in the text of the article.

The *horizontal and vertical scales* for a graph should be chosen with care, so as to give a correct impression of the relationship plotted, for the choice of scales has a controlling influence on the apparent rate of change of the dependent variable. Except where a visual comparison of plotted magnitudes is important, the bottom (abscissa) and extreme left-hand (ordinate) coordinate lines need not represent the zero values of the variables plotted: this often results in a more effective graph as well as a saving of space.

The numerals representing the scale values should be placed outside the grid area. If the scale values are smaller than unity and are expressed in decimal form, a cipher should always precede the decimal point; thus, 0.20, not .20.

The use of many ciphers in scale numbers should be avoided, and the best way to do this is to reexpress the quantity plotted in terms of a larger unit of measurement. For example, suppose that originally the scale numbers are 15 000, 20 000, 25 000 . . . and that the scale caption is "Pressure (lb/in.^{2})"; these scale numbers can be changed to 15, 20, 25 . . . , provided that the unit is changed to 10^3 lb/in.^{2} If, in this example, the data are correct to three significant figures and it is desirable to indicate this fact, then the scale figures should be 1.50, 2.00, 2.50 . . . , and the unit, 10^4 lb/in.^{2} *Never* use captions of the types: "Velocity $\times 10^3$ in ft/sec" and "Velocity (ft/sec $\times 10^3$)." They are ambiguous, since they do not indicate clearly whether the scale numbers *have been* or *are to be* multiplied by 10^3.

Book Reviews

In general, unsolicited book reviews are not considered for publication. The editors reserve the right to reject solicited reviews.

Anyone who undertakes to prepare a book review has accepted certain obligations: to the author, to the publisher, to the editor, to the reputation of the journals, and especially, to the reader.

The reviewer should consider what a reader might like to know about a book. Is it a good book of its kind? In what way is it better or worse than its predecessors? What field does it cover? To what audience is it addressed? If the book is written for a popular audience, the reviewer should judge how successful it is for that audience. Would it be a good book to own? How well does it fulfill the stated aims of the author? If it is not a first edition, how has it been changed, if at all?

The reviewer owes it to the author, who has undoubtedly spent much time and effort on the book, to be fair. He should not magnify minor errors out of proportion to their importance, but he should point out without rancorous or polemical outbursts any weaknesses, important errors, or misconceptions. In short, the reviewer should give an appraisal of the book, not of the author. If it is a generally good book with some faults, or a generally bad book with some good points, this should be made clear to the reader. If the book lies outside the field of the reviewer's competence, which may mean that its title is misleading, he should return it to the editors, or, if someone he knows is competent both in the field of the book and in writing reviews, he should turn the book over to him for review and *notify* the editorial office. If the book does not merit a review, the editorial office should be notified promptly.

Promptness in a book reviewer is an especially desirable virtue. The longer the lapse between publication of a book and its review, the less valuable the review.

Reviews should be no longer than necessary. An optimum length is between 200 and 300 words, an approximate upper limit is 650 words. The length of a review need not be proportional to the merit of a book. In fact, the relation may be one of inverse proportion, for a book with some merit and many defects may require a long review.

The writing should be clear and concise, and the reviewer should remember that he is writing for some nonscientists as well as for specialists in one or another field of science. The reader should be able to tell whether or not the books reviewed in fields other than his own have merit. A librarian, for example, should be able to decide from the review whether or not the book in question should be purchased for library use.

News of Science

Nuclear Tests

The U.S. Atomic Energy Commission announced on 2 Apr. the sixth atomic explosion by the Soviet Union in the last 8 months. A Soviet test series, which included one large hydrogen blast, started last summer. Three smaller explosions were detected in August and two more have been reported this spring.

The announcement followed by a few hours word from the AEC that 15 newsmen and Civil Defense officials will be permitted to observe a United States nuclear test at the Pacific proving grounds at Eniwetok about 1 May. The AEC spoke of it as a "megaton range nuclear detonation." One megaton is the equivalent of 1 million tons of TNT.

The United States set off a hydrogen explosion at Eniwetok in March 1954 with a force reportedly equivalent to 14 million tons or more of TNT. By comparison, the Hiroshima atomic bomb yielded the equivalent of 20,000 tons of TNT.

The State Department has sent a note to the Japanese Government concerning the Pacific nuclear tests that includes the following statements.

"The United States is second to none in its desire for the safeguarded control and reduction of armaments, including nuclear weapons. . . .

"The United States Government is convinced that the proposed nuclear tests are vital to its own defense and the

defense of the free world because the possession and competence in the use of nuclear weapons by leading nations of the free world are the chief deterrent to aggression and to war. . . .

"The United States Government is convinced that no world-wide health hazard exists from the past or planned tests. In this connection the United States proposed a resolution unanimously adopted by the United Nations Tenth General Assembly establishing a scientific committee on radiation, of which Japan is a member, to facilitate pooling and distribution of all available scientific data on the effects of radiation upon man and his environment. During the forthcoming tests the United States will make every effort to eliminate any danger and to minimize any inconvenience to maritime commerce and fishing. . . .

"In view of precautions which will attend the tests and the widespread dissemination of information with respect to maximum permissible levels of radiation, the United States Government anticipates no economic losses from radioactive contamination of marine life."

Meteorological Buoy

A robot marine weather station, so compact it can be anchored like a navigational buoy to send weather data from remote areas, has been developed by the National Bureau of Standards. The buoy can be left unattended for as long as 6 months. Preliminary tests in Chesapeake Bay show that the station has a radio range of more than 800 miles. If moored in certain areas of the Caribbean, it might give warning of forming hurricanes.

The station is 20 feet in length and 10 feet wide, with a draft of slightly more than 3 feet. It is constructed of aluminum and other nonmagnetic alloys.

The vessel, which has two masts, can be anchored in waters as deep as 3600 feet. Four watertight wells extending below the deck hold all the electronic and meteorological equipment assembled in compact, shock-mounted units.

U.N. Economic and Social Council Agenda

The 21st session of the United Nations Economic and Social Council opened in New York on 17 Apr. Scientific items to be considered, as they appear on the provisional agenda, are as follows.

International cooperation on cartography. The council will have before it a report of the Secretary-General emphasizing three points on cooperation in this field.

The first deals with the United Nations Regional Cartographic Conference for Asia and the Far East which was held at Mussoorie, India, in February 1955 and with the recommendations adopted there which are of particular concern to the United Nations. The recommendations were that the United Nations assist governments in the solution of various problems, such as limits of mapping responsibility for the International Map of the World on the Millionth Scale, first-order triangulation and leveling connections between neighboring countries, and gravity observations at sea, as well as strengthening cooperation in cartography.

The second point refers to continued consultations with governments on the adoption of a standard method of writing geographic names on maps, as requested by the council at its 15th session.

The third point concerns means to further the completion of the international one-millionth map of the world, as requested by the 15th session of the council.

International cooperation with respect to water-resource development. At its 18th session, the council considered an interim report which noted that in many areas the supply of water is proving inadequate to meet the growing demand, that this is a deterrent to economic growth, and that the situation calls for full knowledge of all water resources and for their management in a fashion permitting maximum benefits from them. The interim report singled out a few areas in which it was felt that international action should be taken and examined ways and means of coordinating activities at different levels.

The suggestions of the interim report met with wide acceptance in the council and led to the adoption of a resolution which requested that the Secretary-General pursue along the proposed lines the endeavors toward strengthening international technical cooperation and report to the council on the results with recommendations on further appropriate action.

This report of the Secretary-General reviews the progress made during the past 2 years at the international and regional levels and singles out some important problems for further consideration by the United Nations.

World calendar reform. In the summer of 1954, the council discussed a proposal by India that the United Nations should adopt a plan prepared by the "World Calendar Association, Inc." for the reform of the Gregorian calendar. The objective of the plan is the adoption of a new universal and invariable calendar based on astronomical data and on the movement of the earth around the sun.

The council asked the Secretary-General to obtain the views of governments on the desirability of calendar reform and placed the matter on the agenda of its 19th session, in the spring of 1955. At its 19th session, the council decided to defer the question to its 21st session.

Studies on atomic energy as a factor in economic development. The United States is proposing this item, as a matter of urgency, with a view to having prepared for submission to the council at an early session an analysis and evaluation of reports and materials available concerning the possible uses of atomic energy for purposes of economic development, particularly of underdeveloped countries.

Convening of an international conference on the creation of new resources of energy and materials. This item has been placed on the supplementary list at the request of France. In an explanatory memorandum, France explains that a general raising of the standards of living cannot be brought about simply by the conservation of existing resources but depends, above all, on the creation of new resources. These resources can be created (i) either in the field of energy, by the rational use of new sources of power (atomic energy, solar energy, wind energy, tidal energy, and so forth), or (ii) in the realm of materials available by the investigation of natural resources that have not yet been exploited (new materials derived from the sea, and so forth), and, more generally, by the development of methods that will enable man to control natural conditions (techniques concerned with artificial rain, soil improvement, cultivation without soil, and so forth).

AAAS Theobald Smith Award

Nominations are requested for the AAAS Theobald Smith award of $1000 and a bronze medal, which has been given yearly since 1937 (except for a lapse during the war years) by Eli Lilly and Company under the auspices of the AAAS. The award will be presented at the association's 123rd meeting in New York, 26–31 Dec.

The prize is given for "demonstrated research in the field of the medical sciences, taking into consideration independence of thought and originality." Any U.S. citizen who was less than 35 years of age on 1 Jan. 1956 is eligible. Research is not judged in comparison with the work of more mature and experienced investigators. The vice president of AAAS Section N–Medical Sciences and four fellows will form the committee of award.

Nominations may be made by fellows of the AAAS. Six copies of all data to be

submitted should be sent *before 1 Sept.* to the secretary of Section N, Dr. Allan D. Bass, Department of Pharmacology, Vanderbilt University School of Medicine, Nashville 5, Tenn.

Idaho Reactor Damaged

The U.S. Atomic Energy Commission has announced that the core of the experimental breeder reactor No. 1 at the National Reactor Testing Station in Idaho was damaged in an experiment to determine the behavior of the reactor during sudden power increases. On 29 Nov., during a planned power surge in the course of the final experiment of a series, a verbal instruction to shut the reactor off was misunderstood, and as a result enough heat was produced to damage the core.

Some radioactivity was released within the building, which was promptly evacuated. No significant radiation exposure or damage other than to the core occurred as a result of the experiment. The full extent of the damage to the core will not be determined until it can be removed and examined. It is believed that the heat generated was sufficient to cause at least partial melting.

Work is underway to remove the core. It is believed that portions of the core structure will have to be replaced to put the reactor into operating condition if additional experimental work with the reactor is considered desirable. The reactor was designed and is operated by Argonne National Laboratory.

News Briefs

■ The Atomic Energy Commission has announced formation of a Committee for Uranium Isotopic Standards that will work toward establishment of national AEC-certified standards. Standards will be established on the basis of U-235 and U-238 content and of total uranium content. Based on this group of primary samples, the AEC will prepare and distribute certified samples. Generally recognized standards of this sort will be of value both in Government operations and private industry for a variety of comparison purposes associated with analytical work.

■ West Germany's first refrigeration and speed laboratory for testing airplane equipment has recently been completed at Brunswick under the auspices of the German Research Authority for Aviation.

■ Ornithologists at the Cornell University Laboratory of Ornithology have succeeded in recording for the first time the voices of two very uncommon birds, the whooping crane and the trumpeter swan. The whooping crane is North America's second rarest bird and is nearly extinct; the trumpeter swan is the fourth rarest. Both species often call in duets, the female adding her voice to the trumpeting of the male. The Cornell group had already recorded the voice of the rarest species, the ivory-billed woodpecker. Third in rarity, the California condor, is believed to be nonvocal.

■ The Wix auditorium of the Weizmann Institute of Science, Rehovoth, Israel, was dedicated on 3 Apr. in the presence of 500 persons who were attending the opening of the International Symposium on Macromolecular Chemistry. The $500,000 auditorium is the gift of Michael Wix, British cigarette manufacturer. It was built for the symposium, which was the first international conference on science held in Israel.

Scientists in the News

ROGER W. RUSSELL, professor and head of the department of psychology at University College, London, England, has been appointed executive secretary of the American Psychological Association. Before going to London in 1949, Russell had been associate professor of psychology at the University of Pittsburgh. He will return to the United States in July.

Russell replaces FILMORE SANFORD, who has resigned to become associate director for scientific studies of the Joint Commission on Mental Illness and Health. The commission was established as the result of special legislation that permitted the appropriation of $1,250,000 in Federal funds to support nationwide studies of mental health.

ROBERT L. CLARK, formerly of the Office of Defense Mobilization, has joined the National Science Foundation to serve as executive secretary of the National Committee for the Development of Scientists and Engineers [*Science* **123**, 662 (20 Apr. 1956)]. In establishing the committee on 3 Apr., President Eisenhower said: "The National Science Foundation will provide staff services for the Committee and provide leadership to other departments and agencies in carrying forward activities which will contribute to a solution of the problem."

ALBERT G. HOGAN, professor emeritus of animal nutrition at the University of Missouri, received the $1000 Osborne and Mendel award of the American Institute of Nutrition at its annual meeting on 18 Apr. He was honored "for his development of synthetic rations for use in nutritional studies and for his original investigations in the field of biochemistry and nutrition, which have made him one of the greatest contributors to the development of our present knowledge of animal nutrition."

DORLAND J. DAVIS has been appointed associate director of the National Institute of Allergy and Infectious Diseases, Bethesda, Md. In the newly established post Davis, who has served since 1954 as chief of the Laboratory of Infectious Diseases, will be responsible for the planning, organization, and direction of the institute's research program in Bethesda and its field station in Hamilton, Mont.

RAYMOND W. HESS of the National Aniline Division, Allied Chemical and Dye Corporation, has received the Jacob F. Schoellkopf medal of the Western New York Section of the American Chemical Society "for outstanding contributions to the solution of the chemical, social and economical problems associated with the control of pollution, and for devoted service to the Western New York Section as officer, committeeman and historian."

CLIFTON G. FOUND, for 9 years research director for the General Electric Lamp Division, Cleveland, Ohio, and its technical adviser since 1951, retired from active service on 30 Mar. He has served G.E. for more than 40 years.

A native of Canada, Found attended the University of Toronto, where he received both his B.A. and Ph.D. in physics. He joined G.E. at Schenectady in 1916, where he was put in charge of lamp research in 1936. He transferred to Nela Park, Cleveland, in 1942 to serve as research director in what is now the Advanced Lamp Development Laboratory. He has written many scientific articles and holds a score of patents. Found expects to reside permanently in Bay City, Mich.

WILLIAM H. ARMISTEAD, director of the research and development division at Corning Glass Works, Corning, N.Y., has been elected a vice president of the company.

GEORGE N. PAPANICOLAOU, professor emeritus of clinical anatomy, Cornell University Medical College, has been selected as the recipient of the $5000 Passano Foundation award for 1956. He will receive the award in Chicago on 13 June during the week of the American Medical Association convention. He is being honored for his fundamental researches in exfoliative cytology, now widely applied in the early detection of cancer, especially of the uterus.

GEORGE E. KIMBALL, a chemical physicist and pioneer in operations research, will join the staff of Arthur D. Little, Inc., Cambridge, Mass., as science adviser on 1 July. Kimball, a member of the Columbia University faculty since 1936, will collaborate on the general guidance of the company's scientific activities.

YADOLLAH SHARIF, chief of the veterinary section, and RAHMA TOLLAH DEYHIMY, chief of the food inspection section of the animal affairs department of the Iranian Army, arrived in Washington, D.C., on 2 Apr. for a 6-week tour of installations of the Army, the Public Health Service, the Quartermaster Corps, and the Department of Agriculture.

EDWARD U. CONDON, who is at present serving as visiting professor of physics at the University of Pennsylvania, will be a visiting lecturer in the department of physics at the University of Wisconsin, 25 June–17 Aug. He will teach a course in quantum mechanics and conduct a graduate seminar in solid-state physics.

MAURICE W. HALE has been named director of the division of veterinary medicine at the Walter Reed Army Institute of Research, Washington, D.C. Before joining the Walter Reed staff, Hale was for 3 years in charge of all the U.S. Army veterinary activities in Europe, with headquarters in Heidelberg. He succeeds CHESTER A. GLEISER, who until his assignment to Germany next July will continue in the veterinary division as deputy director.

CHARLES D. SWARTZ of Johns Hopkins University will join Union College on 1 Sept. as associate professor of physics.

Grants, Fellowships, and Awards

■ The U.S. Public Health Service has announced a new procedure to expedite the processing of research grant applications for those requests that do not exceed $2000 plus indirect costs and do not ask support for more than 1 year. Such applications will be accepted and processed on receipt and will not be subject to the usual submission deadlines.

Council recommendations can be expected on these applications from 1 to 4 months from the time of submission. These procedures do not apply to requests for supplements to existing grants. Address all applications, as well as requests for forms or additional information, to the Division of Research Grants, National Institutes of Health, Bethesda 14, Md.

■ The American Heart Association has announced awards totaling $830,000 to 131 scientists for cardiovascular research to be conducted during the 12 months beginning 1 July. These grants raise to more than $14 million the research funds allocated by the Heart Association and its affiliates since the organization became a national voluntary health agency in 1948.

The most recent awards provide for three career investigators, 64 established investigators, and 64 research fellows, who will conduct their studies in 24 states, the District of Columbia, Great Britain, Greece, and Denmark. The career investigatorship provides an unrestricted lifetime support to scientists of unusual accomplishment and outstanding ability. It provides $25,000 per year to cover the stipend of the career investigator and to help meet the expenses of his laboratory.

Established investigatorships are awarded to highly qualified, experienced scientists to conduct an independent program of research. They are given for 5-year periods, subject to annual review, and carry stipends of from $6000 to $9000 per year. Research fellowships, awarded for 1 and 2 years, are intended to help younger scientists become established in research careers. They provide stipends of from $3500 to $5600 per year.

Miscellaneous

■ The first issue of the *Illinois Journal of Mathematics* will be issued early next year. It is financed by part of the $1-million estate left to the University of Illinois by George A. Miller, professor of mathematics, who died in 1951. The American Mathematical Society is joining in the support of the new quarterly, which will be devoted to basic research in pure and applied mathematics.

Each number will contain 150 pages. Papers will be published in English, French, German, and Italian. Editors will be Reinhold Baer, J. L. Dobb, and A. H. Taub of the University of Illinois, George W. Whitehead of Massachusetts Institute of Technology, and Oscar Zariski of Harvard University.

■ Highly qualified scientists are needed for the research and development of air weapon systems for the Air Force. Since June 1953, budgetary restrictions have prevented scientists from receiving a direct appointment in the Air Force Reserve.

As an exception to usual policy, the commander of the Air Research and Development Command, Baltimore, Md., may now forward to the Air Staff applications of outstandingly qualified scientists to be considered for direct appointment into the USAF Reserve and, upon approval, for immediate entry into extended active duty. Those accepted will be commissioned as second lieutenants and will serve a minimum active duty tour of 3 years.

■ The *Journal of the American Medical Women's Association* celebrates its tenth anniversary this month. A special issue includes material from those who contributed articles to the first issue and those who helped launch the journal. "Looking backward," by Esther P. Lovejoy, tells the history of women in medical journalism in the United States. Ada C. Reid, the former editor, describes the early development of the journal in "Those were the days!" Others who are represented are Elise S. L'Esperance, Carroll L. Birch, and Catharine Macfarlane.

Meeting Notes

■ The annual meeting of the North Dakota Academy of Science will be held 4–5 May in Jamestown, N.D.

■ Developments in the fields of radiation and isotopes as they apply to both civilian and military medicine and dentistry were featured at the fourth annual Military-Medical Symposium that met in Boston, Mass., 21–23 Mar. The symposium, which was sponsored by the First Naval District, was made up of a series of meetings and clinics. The first-day sessions were held at the U.S. Naval Hospital in Chelsea. The second- and third-day programs were devoted to clinics and lectures in various Boston hospitals.

Shields Warren, professor of pathology at the Harvard Medical School and a captain in the Navy Medical Corps, was general chairman of the meeting.

■ The third annual Conference for Engineers, sponsored by the College of Engineering of the Ohio State University, will be held in Columbus on 4 May. The keynote address on "The creative age—a challenge to engineers" will be delivered by John R. Hoover, president of the B. F. Goodrich Chemical Company of Cleveland, Ohio. At the luncheon session more than 1000 engineers will hear Dean E. Wooldridge, president of the Ramo-Wooldridge Corporation of Los Angeles, Calif., discuss "Systems engineering."

Following these talks, technical sessions will be held in the various engineering departments. Requests for information and reservations should be addressed to Harold A. Bolz, College of Engineering, Ohio State University, Columbus 10.

Reports and Letters

Wisconsin's Science Writing Program

The need for reporting of scientific events has been recognized by the press and the popular magazines for a long time. A recent survey [*Science* **123**, 95 (20 Jan. 1956)] has shown that articles on scientific subjects are consistently among the most interesting to newspaper readers. Scientists generally have realized the need for wider dissemination and greater understanding of the findings, aims, and methods of science, so that applications can be made quickly for human welfare. Our experience has shown that news of scientific developments often reaches many scientists first through the nontechnical press. For these reasons, accurate popular reporting is important, and many of the larger scientific centers now employ specialists to disseminate accurate nontechnical accounts of research work

To train specialists in science writing, a science-writing research assistantship, supported by the Wisconsin Alumni Research Foundation's annual research grant to the university, was established in 1948 as part of the program of the University of Wisconsin News Service. The assistantship fund is allocated to the news service by the university's research committee, of which Conrad A. Elvehjem is chairman. The holders of assistantships, two of which are provided each year (since 1954), enjoy a wide latitude of expression (press, radio, television), and they may study for advanced degrees in any field of science or journalism. Through the program, Wisconsin has become a pioneer center for the development of skill in this field, for testing old techniques and developing new ones.

All who have held the assistantship so far have been employed later as science writers, two with federal organizations (U.S. Department of Agriculture and U.S. Forest Products Laboratory), one for a university (State University of New York), and one for the Canadian Medical Association. Two assistants held the post longer than 1 year. The second writer trained under the program, Jane (Davidson) Schultz, wrote a paper in partial fulfillment of the M.S. degree (granted in zoology) on readability of technical scientific writing. One interesting phase of her study concerned fairly marked differences in readability between the papers on parasitology appearing in four journals. Technical words and phrases are needed in scientific communication, she points out, but many writers compound reading difficulty by using multisyllable nontechnical words where simple ones would do. Among the examples she found were *multifarious, contemporaneous, cerebration, informational,* and *desideratum.* Summarizing, she wrote: "One, scientists use too many words in their sentences; two, they employ a supporting framework of nontechnical words which are chuck full of syllables."

Much of the Wisconsin science-writing program is devoted to the development of skills in straight reporting of scientific news and in writing feature articles. Most students holding the assistantships have had training in the biological and medical sciences or in science writing in these fields. Personally valuable to them is the field work interviewing scientists, translating scientific reports into news stories and articles, with readers of the popular and semipopular press in mind, checking stories back with scientists, and evaluating the reception of their work in the press.

An account of the assistants' experience with the familiar techniques might be of some interest here. We have made fairly extensive use of the article series —three or four short articles intended for publication over as many days—to describe work in a broad field, such as cancer research. These are picked up widely by the state press and often have been adapted for one use or another by many types of publications. A second technique, one that eliminates any chance that rewriting will result in inaccuracies, is the matting of feature articles, with text usually accompanied by a photograph. Once prepared, such stories are easily cast into type and, for this reason, are particularly well received by small weekly papers.

To my knowledge, Wisconsin has the only graduate program in which research assistants are permitted to specialize in science reporting exclusively and study for a higher degree in any field (thesis research must, however, be carried on independently of the science-writing program if necessary for a degree). The yearly stipend is $1620. It would seem that greater effort to train writers on scientific subjects must be made if we are to progress as rapidly in interpreting science as science has progressed in devising ways to reveal new knowledge of the universe.

JAMES A. LARSEN
University of Wisconsin News Service, Madison

2 February 1956

Use and Abuse of English in Science

The following remarks are part of the leading article that appeared under the title used here and in the same issue of Nature *(5 Nov. 1955) in which John R. Baker's article (see page 713) was first published. This excerpt is reprinted here by permission.*

On the question of the use of a German construction for the sequence of adjectives and nouns, Dr. Baker is probably right in stating that the practice is growing. In some cases—and Dr. Baker quotes examples—only the judicious use of the occasional hyphen can save a phrase from meaning the reverse of what is intended; the meaning may still be obvious from the context, but it should not be necessary to have to go to the context in such short phrases. It is difficult to find any real justification for the use of this style. To a limited extent it may give crispness to a statement and save the use of the word *of*; but its general adoption seems dangerous and unnecessary. Dr. Baker's examples could readily be multiplied, and point to the growth of a new style in scientific writing which has nothing to commend it.

Pasing now beyond Dr. Baker's article, reference may be made to another tendency—perhaps it is only a mannerism—which has been noticed in recent years, namely, an unusual use of some common verbs. Thus an author may write that he "reacted zinc with hydrochloric acid," or he "combusted" something, or that "a metal distorts upon cooling," or that "a wave propagates in a straight line." Such phrases are frequently seen in communications submitted to *Nature,* from Great Britain as well as elsewhere, and sometimes it is difficult to be sure whether or not a new technical meaning is being given to a hitherto common word. Unless some particular quality is intended by the use of common words in this way, it would seem that an unnecessary grammatical change is being introduced. The English language, in spite of its lack of

inflexions, is wonderfully fluid and surely does not need to be subjected to such twists in order to accommodate new terms or to reexpress old ones.

Another problem which is causing increasing concern—to printers as well as to editors—is the frequent and indiscriminate use of abbreviations in the form of a single capital letter, or a group of capitals, to represent the name of a substance, or perhaps even an adjective or adverb. The printer is concerned because a page of text sprinkled with capital letters is not pleasing in appearance; and, like other craftsmen, he feels that his efforts are being frustrated. It may be argued that this is of no concern to the scientist; but surely his work is worthy of good presentation, and there is satisfaction and even advantage in recording the results of research in an elegant style.

So far as the use of abbreviations themselves is concerned, the subject was discussed some time ago in these columns. As was pointed out then, a little consideration will show that abbreviations can often be replaced by pronouns without loss of clarity or accuracy. No one writing for the general reader would willingly use the same noun several times in a single sentence; yet when abbreviations or symbols are adopted, there seems to be no hesitation in repeating them—often as many times as possible. Biochemists are great offenders in this respect, and their excuse must be that many of the substances they use have unwieldy names. Similarly, organic chemists are hampered by a cumbersome nomenclature, which however admirable from the point of view of describing a substance, invites the use of abbreviations; indeed, the industrial organic chemist has found it necessary to invent names or use specific symbols like DDT for the sake of the users of his products. But a sense of proportion must be maintained in devising and using specialized terms; thus it seems pointless to write *p*-ClHgBA for *p*-chloromercurobenzoate, and positively dangerous to use H for histidine. The habit—for it is little else—of introducing abbreviations is now spreading through physical chemistry to physics and to mathematics; indeed, to all sciences. Thus E has been used for *enzyme*, with the derived symbols ES for *enzyme substrate* and ESI for *enzyme substrate inhibitor*, and M.O. for *molecular orbital*; and what is the physicist to think when he finds a plentiful sprinkling of the term MIT on a page, only to find on careful reading that it is an abbreviation for monoiodotyrosine and not the Massachusetts Institute of Technology? Then, again, can the use of ARG for *autoradiograph* be justified? Mathematical signs occasionally appear in a line of text, especially the sign for *is equal to*. These examples are taken from manuscripts submitted to *Nature*, and many others might readily be quoted.

It may be claimed that, if an author writes out the name or term, followed by his chosen abbreviation, on the first occasion of its use, or includes a list of the abbreviations he proposes to use, then he is justified in employing his abbreviations throughout the remainder of that particular piece of writing. But is every scientific communication to have its own glossary? Consider the confusion—and additional space required—which would occur in a journal such as *Nature*, in which two dozen or so specialized communications appear every week. The fact is that these abbreviations are, in the main, laboratory and notebook shorthand, or even slang, very valuable in their proper place, but nevertheless a new language, which at present does not seem suited for the considered statement offered for discussion and appraisement by other workers.

The use of abbreviations, especially initial letters, is now becoming so fashionable among scientists that one suspects authors sometimes go out of their way to use them; for example, SV for *seminal vesicle*. With the increased sliding growth among the various disciplines of science, resulting in such departments of research as biochemistry and biophysics, this fashion may, if not checked, defeat its own ends and produce a veritable "Tower of Babel." Indeed, the time does not seem far away when high-school pupils will have to learn a new table of symbols apart from those atomic.

A further problem, which may not occur to most writers, has to be faced in the preparation of an index. Abbreviations or initials sometimes occur alphabetically in quite a different position from that which would be occupied by a full term, thus causing confusion during reference.

New Look in Soviet Genetics

American geneticists have been aware for nearly 2 years of a change in the attitude toward their science in the Soviet Union, and particularly of the declining influence exerted by T. D. Lysenko and his colleagues on Russian biology. Until recently, most of us were skeptical over the development of any genuine change in attitude that would permit our science to develop as it did in the Soviet Union before 1936. Recent events, however, are showing that the change is a real one, and that Mendelian genetics is apparently reviving there. A report from the editors of "*Drosophila* Information Service" indicates that two outstanding geneticists, M. S. Navashin and N. P. Dubinin, are again coming into prominence and are organizing genetics laboratories in Leningrad and Moscow, respectively.

An additional indication is given by a recent review of the book *Hybrid Corn*, which is an illustrated, 360-page collection of articles translated into Russian by M. L. Belgovsky, Y. I. Lashkevich, and V. V. Khvostova. The book was published in Moscow in 1955 by Foreign Literature Publishers. The review, of which a partial translation follows, appeared in the November–December 1955 issue of *Botanicheskii Zhurnal*, the Soviet counterpart of the *American Journal of Botany*. The reviewer, D. V. Lebedev, is unknown to me. I am very grateful to my colleague, I. M. Lerner, for assistance with the translation.

"Speaking at the conference of genetics and breeding called in October 1939 by the editors of the journal 'Under the banner of Marxism,' and giving a survey of the contemporary situation of world science in these fields of knowledge, Academician N. I. Vavilov dwelt particularly on the use of heterosis in corn breeding. Producing the official data on the areas occupied by hybrids of inbred lines in the U.S.A. in 1938–1939, and on the increase in yield which resulted from the entrance of these hybrids into production, he evaluated hybrid corn as the most valuable practical achievement of genetics. N. I. Vavilov asserted that 'on the basis of genetic investigations which had been carried out by theoretical workers, not practical men or breeders, the theory of inbreeding was worked out on corn material, a theory now widely used in practice.' (Vavilov, 1939: 129). In his lecture he cited a letter from American corn specialists, who wrote that the breeding of this important food and fodder crop, which had marked time in the course of a whole century, as a result of this new work moved forward all at once at an extraordinary rate.

"The time that has elapsed since this address of N. I. Vavilov has demonstrated completely the basic correctness of the high value that he ascribed to the theoretical and practical knowledge of hybrid corn.

"The decisions of the January Plenum of the Central Committee of the Communist Party of the Soviet Union established the task of the wide application of new breeding methods for corn, and the complete transformation of this crop in the course of a few years to sowing hybrid seeds. There is no doubt that one of the conditions for completing the task which has been set is the very rapid use of all the experiments of foreign science, an acquaintance with those works which have been and are being carried on

abroad. The anthology *Hybrid Corn* gives in Russian translation some of the most important investigations and reviews of work which have appeared in recent years."

There follows an extensive review, with uniformly favorable comments, of articles by Mangelsdorf, Shull, Hayes, Ritchie, Wellhausen, Sprague, and others, most of which are well known to American plant geneticists.

"Even from such a very short summary it is clear how extensive and interesting material is represented in the work being reviewed. The editor successfully chose the articles for translation and made very appropriate use of the relatively small space available to him in the book.

"In conclusion, however, two remarks must be addressed to the publishers of the book.

"We have spoken several times about the fine work of the editor of the anthology, but have not mentioned his name. The name of the editor—Corresponding Member of the Academy of Science of the U.S.S.R. N. P. Dubinin—for some reason is concealed from the reader by the publishers. This seems to us completely unpermissible. The editor carried out a large and necessary task, and the reader has the right to know whom he should thank for it.

"The second remark is the following. In a short preface 'From the publishers,' among other things, the following is said: 'Each of these articles contains much valuable factual material; at the same time, since the authors of all of the articles hold to the viewpoint of the chromosome theory of heredity, this material must consequently be used critically.' This sentence calls for legitimate perplexity.

"The publishers, presenting to the reader a book founded entirely, as they themselves recognize, on the basis of the 'chromosomal theory of heredity' [we put this expression in quotation marks, because it is far more correct to say: on the basis of the doctrine of the 'material (or cytological) basis of heredity'], warn that this theory is mistaken. But if it is discarded, then nothing remains in the book. There does not remain the method of inbreeding, which establishes pure lines, hybrids of which should by 1960 occupy the entire 28 million hectares in the U.S.S.R. sown to corn. There does not remain the marking of the parental lines with marker genes, without which pure (homozygous) lines cannot be produced in a restricted period of time. There does not remain the cytoplasmic male sterility, which gives to the country the possibility of saving millions of work days from the labor of removing the tassels. There does not remain the method of selecting gametes, significantly easing and simplifying the breeder's work.

"In fact the only thing which should be discarded in the book is the sentence of the publishers that was quoted.

"We must hope that the publishers of foreign literature in the future will regularly acquaint Soviet readers with all of the more interesting work on hybrid corn and on other problems of contemporary genetics and in this way further the completion of the tasks that have been assigned to our science."

This statement, written solely for Soviet plant scientists, is clear indication of a scientific revolution already in progress that may restore genetics to its rightful position in a great and scientifically progressive nation and that will make future contact between American and Soviet geneticists far easier. Let us hope that it will continue.

G. Ledyard Stebbins
Department of Genetics,
University of California, Davis

22 March 1956

The resignation of T. D. Lysenko as head of the All-Union Academy of Agricultural Science was announced in Moscow on 9 Apr. 1956.

International Geophysical Year Symbol

The Special Committee for the International Geophysical Year (CSAGI) has adopted two symbols in connection with the IGY, shown in Figs. 1 and 2 (kindly provided by D. C. Martin, assistant secretary of the Royal Society).

Figure 1 presents the symbol to be used on CSAGI and other IGY publications, under rules issued by the general secretary on behalf of CSAGI, where no inscription is needed because the pertinent information will already be present on title pages and covers. This symbol attempts to suggest the scope of IGY. For example, the earth is partly light, partly dark—suggesting solar-terrestrial relationships; a satellite and its orbit indicate IGY interests in the physics of the high atmosphere; and the orientation of the earth, showing the South Pole, not only implies the conduct of an unprecedented IGY research effort in that region but may suggest IGY coverage of other regions ordinarily not the subject of extensive geophysical study.

Fig. 1

Fig. 2

The second symbol (Fig. 2) is proposed for use on instruments, equipment, and so forth, where the French or English language is appropriate; other national committees will translate the inscription.

Hugh Odishaw, *Executive Secretary, U.S. National Committee-IGY, National Academy of Sciences, Washington, D.C.*

3 April 1956

Library Searches with Punched-Card Machines

The following example illustrates the problem to be discussed. A chemist asks his librarian to list published articles that contain data on compressibility of sodium chloride solutions. With the conventional card index, the librarian must look under *solutions*, *compressibility*, and *sodium chloride*; and relevant articles may also have been indexed under still other subjects. The search is laborious and may miss important items.

With machine methods (*1*), a different procedure is possible. Each article is represented by a punched card. This *abstract card* contains, in coded form, the identification number of the article, author and journal information, and 10 or 15 key words, or *attributes*. The attributes identify subjects to which the article is related. The abstract cards need not be kept in any special order: a machine can search the whole file rapidly and automatically select relevant cards.

For coding attributes, two methods are possible: each attribute may be punched in a separate small field on the card (*2*), or codes for various attributes may be superposed in one large field. We chose the latter method. Its advantage is that many attributes may be entered on a single card. Its disadvantage is that the coding is irreversible. For instance, if each attribute is coded as a pattern of four holes, then a given set of eight holes can result from the coding of any one of $8!/4!4! = 70$ pairs of attributes. This disadvantage is not serious if only a small fraction of the theoretically possible number of codes is used, and if these are chosen at random. A search will then seldom produce more than a few spurious cards; and it will never miss any relevant ones.

Random coding has been exploited with Zato cards and with McBee Keysort cards (*3*). To apply it to IBM cards, we prepared a set of *master cards* containing four-hole codes. The codes were punched in the field consisting of digit positions 0 to 9 in columns 1 to 40 of

the 80-column IBM card. The number of possible codes is $400!/4!396! \fallingdotseq 10^9$.

A 20,000-card deck of master cards was prepared, by machine methods, from the first 5000 cards of a random-digit deck obtained from the Rand Corporation. Each Rand card contains 50 random digits. The first 48 digits were divided into four equal groups; each such group was used to produce a four-hole pattern. For this purpose the group of 12 digits was subdivided into four three-digit numbers, and each of these was interpreted as a hole. Each three-digit number not already in the range 000–399 was converted to one in that range; then the number 238, for instance, was interpreted as "punch 8 in column 23" (card column 40 was renamed 00).

To preserve the randomness, the conversion was done as follows. Initial 4, 5, 6, 7 were replaced by 0, 1, 2, 3, respectively; initial 8 by 0 or 1; initial 9 by 2 or 3. The approximately 8000 choices between 0 and 1 were made at random, by use of the 5000 random digits in column 49 of the Rand cards; the choices between 2 and 3 were made by use of column 50. The first 5000 of the 8000 choices were determined by whether a digit was in the set 0, 1, 2, 3, 4 or in the set 5, 6, 7, 8, 9. The remaining choices were determined by whether a digit was in the set 0, 1, 8, 9 or in the set 3, 4, 5, 6; digits 2 and 7 were rejected.

The abstract cards are produced as follows. A scientist reads the printed abstract and lists the attributes to be coded: for example, *solution, compressibility, sodium, chloride*. These are taken from a standard list or, when necessary, added to it. A machine operator does the rest. Each attribute on the list has a serial number, which is also punched on the corresponding master card. Master cards with the specified serial numbers are selected by machine methods. For each abstract, the codes are reproduced first from the master cards to separate detail cards, then from each detail card to the next, until finally a single card contains the superposed codes for all the attributes.

A search can be made as follows. When n attributes have been specified, the corresponding pattern of holes ($4n$ or fewer) is determined. The entire file is put through the sorter once, to select cards punched with a specified hole. The selected cards (a small fraction of the total) are put through again, to select cards punched with a second specified hole; and so on. A search can be made with a *single* pass through a more elaborate machine; but the sorter is fast and inexpensive.

To test the coding and search procedures, about 2500 abstracts, published by the American Petroleum Institute, were indexed; 377 of these were converted into abstract cards (*4*); 1700 attributes were needed. The coding and punching were done twice, independently, and the cards were compared in the reproducer. When two persons with different backgrounds (chemistry and physics) indexed independently and then compared, abstracts were converted to cards at the rate of 2.3 per man-hour. When only one person indexed, the rate was 3.0 per man-hour. One-word attributes worked well. The work of coding and punching additional attributes was outweighed by the flexibility gained in indexing; new concepts could be indexed more precisely.

Several trial searches were made with the 377 cards. No difficulties emerged, and in one search an abstract was found that had been missed in the conventional search. The file was too small to permit reliable estimation of searching time.

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4. We gratefully acknowledge the valuable assistance of Kate C. Ornsen, Margaret Johnson, and Carl W. Dempsey.
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9 January 1956

Dangerous Dagger

Lately this laboratory received from abroad a note of commiseration on the sudden death of our distinguished director, who, happily, is much alive. Misunderstanding apparently arose from the heading of a published paper, where the author's name was followed by a dagger (†).

On the continent of Europe the dagger thus placed is almost universally understood to indicate posthumous publication. However, many American editors use it for a footnote giving the author's professional connection or other information. This happens commonly when an asterisk follows the title. Would it be too revolutionary to replace the dagger by some other symbol or a double asterisk?

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6 January 1956

Subject Indexing in a Restricted Field

Uriel H. Schoenbach's letter on "Citation indexes for science" [*Science* **123**, 61 (1956)] contains one of the best solutions to the serious problem of assuring efficient information retrieval from the scientific literature. His suggestion that existing indexing techniques be greatly expanded in order to cover all of the significant aspects of scientific publications, instead of stressing only the "highlights," is certainly very well taken and, if implemented, would greatly facilitate the more efficient communication of data.

The Chemical-Biological Coordination Center of the National Academy of Sciences–National Research Council collects, abstracts, and codes information concerning the effects of chemical compounds upon biological systems (*1*). These data are processed, stored, and retrieved by the use of machine-sorted punch cards. Publications to be abstracted and coded are carefully examined by highly qualified scientists and subjected to the sort of detailed and discriminating indexing that Schoenbach has in mind (*2*). As a result, there may be as many as 20 separate approaches (index entries) to a particular paper (*3*).

Recently, a Cardiovascular Literature Project was set up at the CBCC, supported by research grant H-2045 of the National Heart Institute, National Institutes of Health, U.S. Public Health Service. One of the aims of this group is to collect, index, and publish all the available information in the world literature concerning the effects of chemical agents upon the cardiovascular system.

The Current List of Medical Literature of the Armed Forces Medical Library performs an excellent and indispensable service not only to the medical community but to documentation as a whole. The indexing and publication techniques employed in the present undertaking are based largely on similar methods that have been evolved and successfully used by the Current List. As a result of the restricted subject matter encompassed by the project, a much more detailed type of indexing can be employed. It is estimated that the "average" publication thus far examined by us contains about ten index entries (*4*).

All chemical compounds tested for any cardiovascular effect are separate index subject headings (generic names and *Chemical Abstracts* names are used). For biological entities, a list of specific subject headings, with ample cross-references, similar to that compiled by the Current List (*5*), but in greater detail, is used.

The published multi-indexed bibliog-

raphy will appear as a series of separate volumes, each covering a specific chronological period. It is planned that the first of these will encompass the world literature from 1951 to 1955, inclusive. The success of this undertaking should provide practical confirmation of the validity of Schoenbach's observations. Other alternatives exist, but it is difficult to find a better practical solution to the problems of adequate bibliographic control at the present stage of development of documentation research.

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6 March 1956

Age and Productivity among Scientists

This paper is concerned with the output of scientific papers among a group of scientists all of whom reached the age 70 and many of whom lived to age 80 or beyond. Among the topics examined are the following: What is the relative productivity of a scientist at various decades of life? What percentage of his total bibliography is produced by age 30, age 40, and so forth? How many contributions are made during the additional decade that is allotted to the octogenarians?

Because my bibliographic source provided data only for the 19th century, it was necessary to choose for study scientists whose adulthood fell entirely between 1800 and 1900. In order to obtain subjects, I selected from the biographical directory of *Webster's New International Encyclopedia*, 1930 edition, each scientist listed therein who lived to age 70 or beyond and whose years of life from age 20 onward fell between 1800 and 1900. This procedure yielded the names of 156 scientists. Of these, 100 lived to ages 70 to 79, inclusive, while 56 lived to ages 80 to 89. The few who survived to age 90 or beyond are not treated in this report.

Table 1. Mean number of papers per person per decade.

Decade	20's	30's	40's	50's	60's	70's
70-year group	9.1	20.1	21.8	23.8	18.1	
80-year group	6.9	21.9	24.7	18.5	17.0	13.1
Combined groups	8.1	20.7	22.9	21.9	17.7	

The 156 subjects belonged to a variety of scientific specialties. There were 17 astronomers, 24 chemists, 19 geologists, 17 mathematicians, 34 naturalists, 15 physiologists, and 20 physicists, while 10 fell into other categories or were difficult to classify. In general they were eminent men in their respective fields, and many are universally famous.

For each subject, a count was made of the number of his scientific publications per decade of life as listed in the *Catalog of Scientific Literature, 1800–1900*, prepared by the Royal Society of London. This catalog lists only papers published in scientific journals and in the proceedings of scientific societies. It does not list other publications, such as books, letters to editors, memorial addresses, obituaries, popular writings, and so on. Thus we are not dealing with complete bibliographies but only with scientific periodical literature. It is believed, however, that the major part of the bibliography of science consists of this kind of publication.

For convenience, the group living to ages 70 to 79 is called the 70-year group, and the remainder is called the 80-year group.

Table 1 shows the mean number of papers published per man, per decade, for each group and for the combined groups. This table indicates that productivity between ages 20 and 29 is quite low. The low productivity of this decade is the result, in large part, of the very low productivity between ages 20 and 24. Of the 156 subjects, 96 did not begin to publish until age 25 or later. However, even the second half of the decade of the 20's does not equal the record of later productivity.

In the 30's a high average rate of productivity is reached, and this rate is maintained for three decades. On the whole, there is little change in mean output of scientific articles between age 30 and age 59. The mean output of my subjects during this period approximates two publications per year. The rate of publication for the combined groups decreases about 20 percent in the 60's, and the 80-year group shows a still further decline in the 70's, although an appreciable amount of productivity is maintained. It will be noted that the number of publications appearing in the 70's is considerably higher than the number in the 20's.

Although the statements just made indicate the general trends, there are, of course, individual exceptions to these. The range of productivity, within each decade, and over the total life span, is great, even for this group of highly eminent men. The distributions are skewed to the left, resembling the upper end of a normal distribution curve. Because of the nature of the distributions, the usual measures of variability are not appropriate and, hence, are not presented.

Table 2. Percentage of total output completed by various ages.

Age	30	40	50	60	70	80
70-year group	10	32	56	80	100	
80-year group	7	28	52	70	87	100

Next, my data are treated so as to show the proportion of the eventual bibliography that is produced by the close of each decade. In the figures that follow, it should be noted that the bibliographies of the 70-year group were closed at age 70 and those of the 80-year group at age 80, as in Table 1. However, the percentages obtained would be only slightly altered if we based them on bibliographies at death rather than at ages 70 and 80. The results obtained are shown in Table 2. It will be observed that a very small part of the lifework of these men was completed by age 30. About one-third of their publications had appeared by age 40, It is notable that nearly one-half of their output appeared after age 50.

Between ages 20 and 70 members of the 70-year group produced a mean of 92.8 papers each. Between ages 20 and 80 the 80-year group published a mean of 102.2 papers per person. This comparison suggests that, for a man of the calibre with which we are dealing, an additional decade of life beyond age 70 results in the production of approximately 10 scientific papers.

Another way to examine this problem is to compare the record of the 80-year group at age 70 with its own record at age 80. Between age 70 and age 80, the mean bibliography of the 80-year group increased from 89.1 to 102.2, a gain of 13.1 papers.

In an earlier paper (*1*), correlations between the degrees of productivity in different decades were computed for two groups of scientists living to age 70. The

Table 3. Correlations between degrees of productivity of 56 octogenarians.

Age in decades	Age in decades				
	30's	40's	50's	60's	70's
20's	.57	.46	.46	.35	.33
30's		.49	.50	.47	.55
40's			.80	.75	.62
50's				.65	.61
60's					.84

same methods have been applied to the 56 octogenarians of the present group with the results shown in Table 3.

The figures are in substantial agreement with those published earlier. The number of scientific publications in the 70's is closely related to productivity in the 60's. The data show also appreciable relationships between the 70's and decades prior to the 60's, the correlations becoming smaller as the decades are farther removed from the final decade.

Readers who are familiar with the various works of Lehman, recently assembled in a single volume (*2*), will note that my findings are at variance with the general tenor of Lehman's results. However, my findings do not necessarily contradict Lehman's. I have dealt with the total output of scientific articles, whereas most of Lehman's attention has been given to "significant" works. In the instances in which Lehman has analyzed the total output of scientists, each of whom lived to a specified age, he, too, finds that productivity persists in the later decades of life.

Finally, it should be noted that the data here presented on eminent 19th-century scientists are in agreement with those that I have previously presented for members of the National Academy of Sciences and for unselected American psychologists (*1*).

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18 November 1955

High-School Students as Laboratory Assistants

In a recent letter [Science **123**, 185 (3 Feb. 1956)] James G. Busse pointed out the advantages of giving summer jobs in laboratories to promising high-school students. A program of this kind was carried out successfully in the summer of 1955 at the University of California, Berkeley, in the departments of biochemistry and physiological chemistry. Results were very encouraging in that students and employers were well satisfied. The students were enthusiastic, hard-working, and intelligent laboratory assistants—well worth the time and cost of training. I hope that scientists in other parts of the country will give a trial to hiring high-school students for the summer.

Currently there is a stress on training more scientists (and engineers). The problem actually is to attract able people and not simply to produce more scientists. I believe the latter aim is wrong, because scientists cannot be mass-produced any more than can competent musicians or athletes. There is danger that if we strive simply for quantity of scientists, the level at which science must be taught will be lowered to a point where the education of the truly promising students will suffer. Effective progress in science must be based on good training; and an opportunity for laboratory experience (to demonstrate the actual nature of science) early in the young scientist's career seems to me one of the best ways of commencing his education and of attracting him into science.

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9 March 1956

Verifying Useless Knowledge

As a collector of useless knowledge, I was entranced by the title of the editorial in *Science* for 2 Mar. but was distressed to see Ben Franklin's devastating remark to the Practical Man attributed to Faraday. Lest noncollectors of useless information be misled by Stanley's lapse, let us check our sources and references. Perhaps Faraday *did* say: "Of what use is a newborn baby?" If so, he stole it from Ben without giving him credit, a very un-Faraday-like act.

James Parton in *The Life and Times of Benjamin Franklin* (vol. 2, pp. 514–515) quotes a brief exchange between a practical-minded spectator and Ben. The time was 21 Nov. 1783. Two Frenchmen, Pilatre de Rozier and the Marquis d'Arlandes, had just taken off from the Tuileries in the first free-balloon flight in history. Says the practical defender of "contract" and "project" research: "What is the use of this new invention?" Answers Ben: "What is the use of a newborn child?"

In the *Harvard Classics*, Emerson writes: (*English Traits, Aristocracy*, page 425) "Loyalty is in the English a sub-religion. They wear the laws as ornaments. . . . The economist of 1855 who asks, of what use are the lords? may learn of Franklin to ask, of what use is a baby?"

Of what use is useless knowledge? has ever been the province of semantics, epistemology—and basic inquiry in anything for the Shopes, the Flemings, the Roentgens, and the fathers and mothers of all newborn babes.

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9 March 1956

Committee on Mathematical Biology

We are disturbed by the drastic reductions that have been imposed on the Committee on Mathematical Biology, headed by N. Rashevsky at the University of Chicago. We wish to point out that the work of this department, the only one of its kind in the world, is of great interest and importance in our diverse fields of research, that is, in biology, clinical medicine, mathematics, psychology, philosophy, and sociology. We feel that it would be a loss if that work were seriously reduced.

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5 July 1955

To write well, even to write clearly, is a woundy business, long to learn, hard to learn, and no gift of the angels.—JOHN GALSWORTHY, Foreword to Hudson's *Green Mansions*.

Book Reviews

The Development of Academic Freedom in the United States. Richard Hofstadter and Walter Metzger. Columbia University Press, New York, 1955. xvi + 527 pp. $5.50.

Academic Freedom in Our Time. Robert M. MacIver. Columbia University Press, New York, 1955. xiv + 329 pp. $4.

The freedom of science and the academic freedom of the university professor are closely interwoven strands in the fabric of our cultural history. From the disappearance of the independent natural philosophers of the 17th century and until the spread of modern scientific research into the nonacademic laboratories of the government and major industries, nearly all science was fostered in the developing colleges and universities of Europe and America. To the scientist, academic freedom thus embraces his personal freedom as a scientist to work on problems of his own choosing, to follow the threads of investigation wherever they may lead, and to publish with impunity the results of his studies, even if they should offend local pride or interest.

This freedom, the very breath of life to science, the scientist must recognize as equally vital in all other fields of scholarly work. In times past it has often been proved, and will inevitably continue to be proved in the future, that the loss of freedom by his colleagues in the humanistic or social studies imperils the freedom of the scientist, even as the loss of the scientist's freedom has catalyzed the loss of academic freedom on a broader scale.

The story of academic freedom recounted in these two volumes consequently has much to say of the freedom of science—from the time of the 14th and 15th centuries, when humanism became a threat to academic vested interests and the Copernican doctrines became a threat to religious dogma, to the time of World War I, when the noted psychologist and founder of *Science*, J. McKeen Cattell, was discharged by Columbia University because he sent a petition to three Congressmen urging them not to approve a bill that would sanction the use of American draftees on the European battlefields; and further, to the still more recent times of Communist alarums and the American quest for security, when disclaimer oaths, Congressional investigations, and public hysteria have shattered the academic freedom of scientists and all other scholars alike.

The first of the two volumes is essentially historical in character. The second is a polemical argument that in the face of recent events cries out like the *Areopagitica* for an enlightened understanding of the meaning and importance of certain liberties. The first volume, by two authors, manages better than most products of multiple hands to achieve not only a unity of viewpoint but even a certain unity of style. Even so, the coordination has not been perfect; and there is an extended part of Metzger's first chapter on "The old regime and the educational revolution" that, instead of analyzing the educational revolution of the years 1865 to 1900, verges on a purely descriptive treatment of "The old-time college (1800–1860)," although that had already been excellently handled by Hofstadter in the final chapter of part 1. Yet this defect may gladly be overlooked in noting the merits of the book: its solid scholarship, its careful effort to avoid bias, and its due attention to the complex aspects of the origin and growth of our American colleges and universities and to the relative state of academic freedom within them. The much briefer volume by MacIver deals with a still unfinished chapter of our history. Its analysis is not as objective, as indeed befits the thought of a warrior in defense of cherished values. Yet where the first volume for the most part dispassionately instructs, the second often stirs the blood. Its call to action should arouse many a sleeping champion of the freedom of the mind.

Richard Hofstadter, author of the first part of *The Development of Academic Freedom in the United States*, devotes a profoundly interesting introductory chapter to "The European heritage," the story of the rise of the great European universities and their early struggles to secure freedom of mind and freedom of instruction in opposition to the bounds of religious dogma and authority. Slowly the idea of toleration, even for the views of heretics, made way, although in the early stages of the foundation of colleges in this hemisphere sectarianism set rigid limits. Through four succeeding chapters, dealing respectively with the early days of Harvard College and the emergence of Harvard liberalism, with the other colonial colleges, with the struggles of sectarianism and secularization, and finally with the great retrogression of learning in the colleges of 1800–1860, Hofstadter traces the development of the American system of a lay government by boards of trustees and the dawn of the idea of academic freedom.

Of particular interest to scientists of today will be the last section, where it is shown how "the modern idea of academic freedom has been profoundly affected by the professional character of the scholar, by the research function and scientific conceptions of the search for truth, and by the manifold services, aside from teaching students, that are rendered to the community by the great university." Among the notable cases of this period is that of Wolcott Gibbs, one of America's great chemists of the time, who was rejected for appointment to a professorship at Columbia University in 1853 because of his Unitarianism, which in that day was still the subject of fierce religious prejudice in many quarters. Gibbs' rejection was in spite of a provision in the Columbia charter that forbade religious tests for trustees or officers; and it led Samuel B. Ruggles, a trustee of the minority group who favored the appointment, to make a public reply that greatly advanced academic freedom and led to the reform of Columbia University. "It is hardly an exaggeration," says Hofstadter, "to say that Columbia University arose out of the case."

In Walter P. Metzger's portion of the book, the part that is of especial interest to scientists is the chapter that discusses the relationship of Darwinism to the freedom of instruction in the colleges. The American university, in the true sense, from its foundation rested on the twin pillars of the evolutionary controversy and the transplantation to America of the *Lernfreiheit* and *Lehrfreiheit* of the great German universities of the 19th century. Thus Daniel Coit Gilman, who became the first president of the first American university at the graduate level, Johns Hopkins University, appointed Newell Martin, a disciple of Thomas Henry Huxley, as professor of biology, and invited Huxley himself to speak at the university's opening ceremonies. The incompetence of the clergy to speak in matters of science was the obverse of the judicial competence of the scientist to speak authoritatively not only in regard to scientific fact and theory, but also to serve as the sole fit judge of the competence of his scientific colleagues. The growth of this concept has led to an im-

portant development in the matter of academic tenure: the right of any university teacher to be judged for competence and fitness to teach exclusively by his peers.

The German influence, also, was clearly evident in the founding of Johns Hopkins University, which was called "the Göttingen at Baltimore." It was there that the German conception of a university as a research institution devoted to the extension of knowledge, even more than to the transmission of knowledge, was first introduced into American higher education. And it was no accident that, with such antecedents, it was from the Hopkins, much later on, in the days not long before the United States entered World War I, that the invitation went out to nine other leading universities to help in the founding of the American Association of University Professors. The latter-day history of academic freedom in this country is chiefly the history of that organization's successful efforts to codify the principles of academic freedom and tenure and, after bitter initial opposition from trustees and administrators, eventually to achieve agreement with them in the maintenance of good standards; and the history also of the same organization's less successful efforts to mediate disputes between faculty members and administrative officers and, in cases of flagrant violation of the established principles, to investigate the cases and to censure the guilty administrations.

In the years before World War I, the struggle for academic freedom shifted from its focus around the evolutionist to one centered on the economist and sociologist, in measure as the vehemence of public opinion about the evolutionary philosophy was mitigated and the role of big business in the endowment and trustee management of American colleges and universities increased. World War I shifted the area of struggle to focus on the pacifist and the professor of German extraction—a grim story lightened only by the magnificent gesture of Harvard University in refusing a $10 million bequest carrying a stipulation that the openly pro-German professor Hugo Münsterberg be dismissed from his post. It is interesting that during World War II there were virtually no infringements of academic freedom of the sort that blemish the history of our universities during World War I. Yet the struggle for the maintenance of academic freedom and the tenure provisions upon which it depends was by no means won. With the quest of the American people for security in the disillusionment, hysterical fear, and suspicion of the Cold War, threats of ever greater magnitude arose to imperil academic freedom.

This most recent chapter of the history of the subject is in part related in MacIver's book, which successively considers "The climate of opinion," "Academic government and academic freedom," "The lines of attack on academic freedom," "The student and the teacher," and "The university and the social order." But as these chapter headings show, MacIver is chiefly concerned with causes and relationships, with trends and general principles. The actual history of these recent episodes is only now being recounted by the American Association of University Professors, which has asked a Special Committee (under my chairmanship) to survey all the cases arising from the national quest for security and to report on them. Beginning with the famous University of Washington "Communist" cases and the dismissals of the nonsigners of the disclaimer oath instituted by the Regents of the University of California, the roll is a lengthy one.

What is of more importance than the judgments rendered in the individual cases of those scientists and nonscientists involved is the formulation of new principles around which the guardians of academic freedom can rally. Perhaps in time, though bitterly opposed by many at first, these principles will come to be accepted by administrative officers and faculty members alike, to stand alongside the 1940 principles as the voice of wisdom in the right regulation of universities and the maintenance of freedom of thought and instruction among scholars and research workers. Having weathered the storm of suspicions, accusations, and investigations, the freedom of the mind that tolerates sharp differences of opinion and encourages criticism—the freedom of the mind that has its stronghold in our universities—will continue in the spirit of John Milton, Thomas Jefferson, and John Stuart Mill, to serve our people infinitely better than the repressed and constrained thought of those who are subject to the bonds of authority.

BENTLEY GLASS
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Soviet Professional Manpower, Its Education, Training, and Supply. Nicholas DeWitt. National Science Foundation, Washington, 1955 (Order from: Supt. of Documents, Washington 25). xxvii + 400 pp. $1.25.

This book reports the results of a comprehensive study of the entire Soviet educational system, with particular emphasis on the training of scientists and engineers. It is based on a large volume of statistical data and descriptive reports from a wide range of sources, and with considerable variation in degree of reliability. The author discusses at some length the difficulties encountered in interpreting and combining available data, which he presents in 45 tables and 11 charts.

Despite these difficulties, this volume is probably the most complete and searching study thus far made of the Soviet educational system, and it seems likely to remain the standard work in the field for some time to come.

Public education is a central purpose of the Soviet Union, but it is based on the needs and desires of the State rather than of the individual. It is interesting to note that, in contrast to the current serious shortage of teachers in the United States, the student-teacher ratio in Soviet schools was reduced from 28 to 1 in 1940 to 23 to 1 in 1950. Furthermore, about 42 percent of Soviet professionals were trained for, and are employed in, the field of education.

Despite the emphasis on education, educational opportunities in the Soviet Union are generally much more limited than in the United States. Soviet higher educational institutions have graduated only half as many persons during the past 25 years as have the higher educational institutions in the United States. However, by sacrificing the humanities and the liberal arts, the Soviets manage with a smaller educational base to turn out a higher number of trained specialists than does the United States.

Throughout the system, a heavy emphasis is placed on technology and basic science. In the elementary grades 1 to 4, inclusive, about 28 percent of the subjects studied are in the fields of mathematics and science. In the intermediate grades, more than one-third of the time is spent on such subjects, and in the secondary school more than 40 percent. The result is that *all* high school graduates have substantial training in and familiarity with physics, chemistry, mathematics, and the earth sciences.

The emphasis on science and technology is reflected not merely in the curriculum, but in various selective devices that tend to channel the ablest youth into the technological branches. Twenty-seven percent of the 2 million professionals in the U.S.S.R. are in engineering and related fields, 16 percent in health fields, 9 percent in agricultural fields, and only 6 percent in all socioeconomic service fields. A similar situation obtains with respect to semiprofessionals.

The Soviet Union is graduating almost twice as many technical specialists in certain fields as is the United States. Between 1928 and 1954, the Soviet Union graduated about 682,000 professionals in engineering as against 480,000 in the United States during roughly the same period. Agricultural graduates in the Soviet Union totaled about 244,000 as against 133,000 in the United States. Soviet graduates in medicine outnumbered those in the United States more than two to one, 320,000 against 148,000.

The Soviets place a much heavier emphasis than does the United States on

the type of training represented in America by technical institutes. There are in the U.S.S.R. something like 1500 of these institutions, which offer training in engineering, science, medicine, pedagogy, law, and agriculture. The courses are generally 4 years in length, with admission being highly competitive and based on the completion of at least 7 years of instruction. Entrance requirements are being raised, and in the near future the Soviets may require a full secondary education. Graduates in recent years have numbered about 60,000 per year in the field of engineering alone, as compared with about 11,000 in the United States.

College-level engineering education in the U.S.S.R. requires many more hours of class attendance than in the United States, and a much greater degree of specialization. Approximately a quarter of the time is spent on the general sciences, and another quarter on general nonspecialized engineering. Only about 6 to 8 percent is allotted for political and socioeconomic courses, which could probably be better called indoctrination courses. Narrow specialized engineering instruction takes the rest of the time except for physical training and military instruction.

The inescapable conclusions to be derived from the book, with all due allowance for possible errors, are that in technological areas the Soviet Union is graduating more trained personnel than is the United States, and that from a purely technical viewpoint Soviet training is probably at least as good as, and quite possibly better than, that received by American engineering and science students.

One ray of comfort may be drawn from the possibility that the extremely high degree of specialization in scientific and engineering fields may carry with it such a lack of flexibility or adaptability that the individual who cannot work in the field of his immediate specialization may be considerably less useful, or perhaps frustrated entirely. However, in view of the high degree of selection in the Soviet educational system, it seems probable that many of those in the field of science and engineering have sufficiently able minds so that this result will not occur. Still, it does represent one possibility that may limit the effectiveness of Soviet technology.

Another possibility is that the great emphasis in the U.S.S.R. on education may lead to changes in the Soviet philosophy. It is conceivable that the emphasis on education may sufficiently emancipate enough minds in the U.S.S.R. so that the seemingly widespread acceptance of the present official Soviet philosophy will no longer be possible.

On the whole, this volume presents a study that is challenging in its significance to American educators, both technical and general, to American industrialists, and to American citizens generally.

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The Soviet Academy of Sciences. Hoover Institute Studies, Series E: Institutions, No. 3. Alexander Vucinich. Stanford University Press, Stanford, Calif., 1956. 157 pp. $2.

One by-product of the cold war has been our realization that what goes on in the U.S.S.R. affects us directly. Thus it is only prudent for us to keep ourselves informed on things Russian and for our scientists to keep a constant check on their colleagues in the Soviet Union. Fortunately, most scientists can learn the status of their specialties in the Communist countries without taking too much trouble, and the scientists as a whole can evaluate the accomplishments of Soviet science quite accurately. We know, for example, that Soviet mathematics, atomic physics, and engineering are truly excellent but that Soviet biology, medicine, and agriculture are backward and permeated by quacks. But over and beyond such information, there exists a mass of background knowledge that can be made available to us only by those who are experts on the Soviet Union. Alexander Vucinich, the author of *The Soviet Academy of Science,* is such an expert.

The work begins with a historical account of the Academy of Science from its founding in 1725 by Peter the Great to the latest tasks imposed on it by the current reigning syndics. The Soviet Academy has now become unique and is quite unlike any academy of science outside the Communist world. Today it plans, organizes, and supervises all scientific research in the Soviet Union. It controls numerous lesser scientific institutions, employs a large staff of scientists and administrators, and dispenses an enormous budget. It has recently acquired a number of new functions, one of which is the training of future scholars and scientists. The individual academicians have great social prestige, receive large salaries, and have many perquisites that are highly desirable.

The academy, however, is subordinated to the Council of Ministers, and the academicians both individually and collectively take their orders from the political authorities. They must be ready at all times to shift their ideology whenever the party line changes. When necessary, they must attack the science and the scientists of the West, rewrite the history of science, and search out the heretics in the ranks of the scientists below them. This latter function is important, for the academicians are responsible for the ideological purity of all Soviet scientists.

The Soviety Academy of Sciences contains a great deal of factual information that is not found elsewhere. It describes the internal organization of the Soviet Academy, its attitude toward pure and applied science, and its role in planning the activities of scientists in the various 5-year plans. The appendixes list the scientific organization of the academy, the different academies of the several Soviet republics, the periodicals published by the academy, and the names of the academicians. The whole is a small but useful compendium. It also gives an exceptionally clear picture of the conditions under which scientists have to labor in the new, socialist Utopia.

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The Mighty Force of Research. Editors of *Fortune.* (15 articles reprinted from *Fortune*) McGraw-Hill, New York, 1956. 308 pp. Illus. $4.

Fifteen articles that appeared in *Fortune* between January 1953 and August 1955 are here brought together in a hard cover. The resulting book gives a well-integrated and easily readable account of the state of applied and basic research and research support by foundations, universities, government, and industry. Several chapters are mainly concerned with technology. The chapter titles give some idea of the scope of the treatment: "The strange state of American research" (support of applied and basic research, research by the General Electric and Bell Telephone Companies, and the role of private foundations and the government); "The young scientists" (a survey by interview of the opinions and characteristics of 20 outstanding scientists under the age of 40, ten from industry and ten from universities, supplemented with the results of a questionnaire of 87 additional young scientists; the generalizations about scientists, derived from so small a sample, are of questionable validity); "Science for sale" (contract research and development with special reference to Arthur D. Little, Inc.); "The inventor in eclipse" (decline of the free-lancer and the rise of the industrial research team; decline in number of patents); "New light on the brain" (an account of recent research on the brain with special reference to H. W. Magoun, H. H. Jasper,

Guisseppe Moruzzi, C. S. Sherrington, K. S. Lashley, W. R. Hess, E. D. Adrian, W. McCulloch, W. Pitts, D. McKay, and Percival Bailey, among others); "How are we fixed for water?" (a survey of water resources; the per capita consumption of fresh water is estimated at 1500 tons annually in the United States); "Tomorrow's weather" (short-range and long-range forecasting; cloud seeding: "For some curious reason most meteorologists have refused to be optimistic"); "Farming's chemical age" (insecticides, herbicides, fertilizers); "Power from the sun" (photosynthesis, solar engines and heating); "The peaceful atom" (atomic power plants, private and governmental); "The new metals age" (light metals and new alloys); "The transistor"; "The automatic factory"; "The information theory" (a clearly developed exposition of the elements of information theory).

There is an index, but no bibliography, which is regrettable inasmuch as the general reader for whom the book is intended might well want guidance to some of the sources for the articles.

In summary, this is a good journalistic job. The dominant note is perhaps more hopeful for the future than it would be in a similar book written by scientists.—G. DuS.

What Is Science? Twelve eminent scientists and philosophers explain their various fields to the layman. James R. Newman, Ed. Simon and Schuster, New York, 1955. vii + 493 pp. $4.95.

Einstein once wrote, "If you want to find out anything from the theoretical physicists about the methods they use, don't listen to their words, fix your attention on their deeds." In *What Is Science?* this advice is taken seriously, not only with respect to physics, but for a variety of fields of knowledge from astronomy through biology to psychoanalysis. This is a wide-ranging collection of essays by distinguished practitioners of the arts of securing their respective kinds of positive knowledge.

A brief review can do no better than to list the contributors: E. T. Whittaker on "Mathematics and logic," Herman Bondi on "Astronomy and cosmology," E. U. Condon on "Physics," John Read on "Chemistry," Ernest Baldwin on "Biochemistry," W. C. Allee on "Biology," Julian Huxley on "Evolution and genetics," E. G. Boring on "Psychology," Clyde Kluckhohn on "Anthropology," and Erich Fromm on "Psychoanalysis." Bertrand Russell, in a prefatory essay, points to the disparity in progress between man's knowledge of the physical world and his values, habits, and beliefs —a disparity that has brought the human race to the brink of possible self-destruction—while Jacob Bronowski supplies a postlude called "Science as foresight," an account of those electronic marvels, the modern calculating machines, that can reason deductively and also explore numerous alternative possibilities within a given logical frame, but, so far at least, cannot trespass on the distinctively human activity of creating new ideas.

What must strike the lay cover-to-cover reader (and what scientist will not be a layman in some of the fields covered?) is the variety of basic concepts in the several sciences and, frequently, their specification to a given subject matter. The "mass-luminosity diagram" identifies astrophysics as definitely as the "subconscious" marks psychoanalysis or "Grimm's law" marks linguistics. The biochemist has his "enzymes," the geneticist his "genes" (of course), the physicist his "particle-wave dualism."

If science has a unity, it seems to lie only in the insistence that theory have observable consequences, that it be susceptible, to quote Bondi, of "empirical disproof." This is not intended to gainsay the remarkable degree of reduction, for example, of astronomy and chemistry to physics, or of some parts of biology to chemistry. Nevertheless, if a new and unique concept or relationship will correlate or explain phenomena, scientists will adopt it in order to carry forward their search for understanding. "Intuitive reactions (such as would cause people to reject implausible conceptions) are of secondary importance," writes Bondi. The testimony for this lies in the voluminous deeds of scientific achievement that are so ably recorded in these essays.

An appended bibliography enables the interested reader to pursue the question, What is science? more broadly and deeply.

AARON SAYVETZ
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Atomic Power. 180 pp. **Automatic Control.** 148 pp. **First Book of Animals.** 240 pp. **The New Astronomy.** 243 pp. **The Physics and Chemistry of Life.** 270 pp. Editors of *Scientific American.* Simon and Schuster, New York, 1956. Paper, $1 each. (Reprints from *Scientific American.*)

Science, like music, has its skilled performers and its audience. With the growing power and importance of science, an increasing number of magazines and books, as well as college courses, are offering the general reader instruction in what might be called science appreciation. The latest venture in this subject is a paper-back series of five books, each on a different field of science, put together by the editors of the *Scientific American* from articles they have published in their magazine since 1948. The result is an account of many recent discoveries that manages to convey something of the method by which scientists find things out, and something of the delight they experience in doing so.

Interpreting research findings to the general public is not the only problem of communication in science today. The rapid appearance of new specialties has made it more and more difficult for one scientist to talk to the next. This series is sufficiently authoritative and detailed to interest scientists who want to know what is happening on preserves other than their own.

The authors of the 90 articles are scientists who can write, many describing their own investigations, and journalists who understand science. Since each article is complete in itself, there is some repetition. But when a topic is new to the reader, explanation in different contexts by different writers may provide understanding where fuller development by one author fails. Of course, browsing is no substitute for systematic study, but the first may lead to the second. Each book has an introduction, some notes about the authors, suggestions for further reading, and a few illustrations—not the original ones, unfortunately.—J.T.

The Biology of the Spirit. Edmund W. Sinnott. Viking Press, New York, 1955. ix + 180 pp. $3.50.

Both author and publisher regard this as an unusual book. And so, in a way, it is. For Sinnott, with his wide knowledge of professional biology, has looked at life and its reasons and justifications, and has concluded that "all problems of life are ultimately biological problems." All living matter, all protoplasm, shows a biological goal seeking, without an appreciation of which it cannot be understood. Now, if once we admit that "life" is the striving of protoplasm to fulfill its goal, we may soon be led to agree that even the mind itself, although it may be "the highest of biological phenomena . . . is a biological process nevertheless"; and then to conclude that "if a man's mind has a biological basis, his spirit must have one also." Protoplasm becomes the place where spirit and matter meet, and "soul is the highest level of that goal-seeking, integrating process that is life."

It is all very nice to find a unifying principle in biology that will help poor human man to relate the tumultuous ex-

periences that he believes to be characteristically human, and it is all very well to be shown how even God himself finds his place as that whose existence must be postulated since without it the highest goal-seeking of truth, beauty, and goodness would be like an ignorant child crying for the moon. There is much in this book that is admirable, but one or two doubts keep intruding. How do we judge what is a "higher" goal or a "lower" one? What makes the goal of the thug or the megalomaniac "better" or "worse" than that of the office boy or the poet? What is the meaning of "goal" itself, unless there is a human mind to experience and judge? And what of the host of evolutionary processes—the dinosaur and the diplodocus—that went wrong? Are we not in danger of failing to recognize that the answers to questions like these lie partly in and partly out of biology? And are we not in danger of forgetting that human wretchedness and human greatness walk hand in hand, and that in the fullness of human living there are undercurrents of blood and sweat and tears that no biology—whether of the spirit or emergent evolution—can properly comprehend? There is something in this book that is well worth saying; but there is still something more to say.

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Biographical Memoirs of Fellows of the Royal Society. vol. I. A New series in continuation of *Obituary Notices of Fellows of the Royal Society.* Royal Society, London, 1955. 263 pp. + plates. 30s.

This new series of brief biographies maintains the high standards of the series it replaces. Each memoir includes a full page photograph of the subject, an account of his life and scientific accomplishments, and a complete bibliography of his publications. They are models of their kind that might well be consulted by anyone faced with the unhappy task of preparing an obituary notice.

Accounts of the lives of the following are presented: Wallace Alan Akers, Herbert Stanley Allen, Richard Stafford Cripps, Arthur Lee Dixon, Albert Einstein, Enrico Fermi, John Theodore Hewitt, James Peter Hill, Frederic Wood Jones, Heinrich Kayser, Arthur Keith, George Martin Lees, John Edward Lennard-Jones, Emmanuel Marie Pierre Martin Jacquin de Margerie, Edward Mellanby, Archibald Read Richardson, Redcliffe Nathan Salaman, George Frederick Charles Searle, and Alan Mathison Turing.—G.DuS.

Imagination's Other Place. Poems of science and mathematics. Helen Plotz, Comp. Crowell, New York, 1955. 200 pp. $3.50.

Although the 20th century seems to have raised a barrier between science and the arts, poets have always seen the same vision as scientists. This anthology attempts to show the reader that Milton went to Galileo for sometime inspiration —that poets and scientists are no strangers. ("Euclid alone has looked on beauty bare," said the late Edna St. Vincent Millay.)

The book is divided into four sections, the first on astronomy and geography. Here man's speculation about his universe ranges from Shakespeare's

The heavens themselves, the planets and this center
Observe degree, priority and place. . . .

to Emily Dickinson's wondering

. . . why heaven did not break away
And tumble blue on me.

Among the poems in the second section, on physics, mathematics, and chemistry, one finds in Carl Sandburg's "Arithmetic" the waggishly profound lines

Arithmetic tells you how many you lose or win if you know how
many you had before you lost or won.

In this section, the least adequate of the four, also appears a paraphrasing of Lewis Carroll's "The Jabberwocky" entitled "Plane Geometry," one of the few choices about which one questions the anthologist's judgment.

In answer to the Psalmist's "What is man, that thou art mindful of him?" the anthology offers the reader the third and best selected group of poems, called "Both man and bird and beast." If the reader is "the egregious egoist" that Elinor Wylie says man is (in "Cold-Blooded Creatures"), he will be delighted with these reflections of himself in the poetic pool. He will be startled by Ogden Nash's

Among the anthropophagi
People's friends are people's sarcophagi

and be awed by Alexander Pope's stern advice,

Know then thyself, presume not God to scan,
The proper study of mankind is Man.

The poems in the last section eulogize the courage, faith, and vision of great men of science, from Newton to Einstein. Here the quality of the poetry is not as good as the spirit that underlies the selection.

Much space is given in the anthology to the modern poets (T. S. Eliot, Dylan Thomas, W. H. Auden, Marianne Moore), but Pope, Wordsworth, Shelley, Emerson, and Tennyson tell the reader how other centuries have seen science.

The book will appeal to readers who see, or want to see, that both scientists and poets attempt to answer the same questions.—J.M.F.

Language, Thought, and Reality. Selected writings of Benjamin Lee Whorf. John B. Carroll, Ed. Technology Press, Massachusetts Institute of Technology, Cambridge; Wiley, New York; Chapman and Hall, London, 1956. 278 pp. Illus. $6.

The hypothesis suggested by Benjamin Lee Whorf that the structure of a person's language is a factor in the way in which he understands reality and behaves with respect to it has attracted the attention of linguists, anthropologists, psychologists, philosophers, as well as a large segment of the public. The present book includes "nearly all of Whorf's writings which are pertinent to . . . the principle of linguistic relativity" (p. 23), as well as other general linguistic writings of interest. The papers are followed by a bibliography of Whorf's published and unpublished writings, and a list of related books and articles. Some of the papers are published for the first time, the most important of these being "A linguistic consideration of thinking in primitive communities" (pp. 65–86).

John B. Carroll suggests, in a lucid introduction, the process by which Whorf's early thinking, at times purely visionary and even mystical, developed into a stimulating and challenging theory of the relationship between linguistic structure and nonlinguistic behavior. Indeed, one cannot help wondering, with Carroll, "what makes the notion of linguistic relativity so fascinating even to the nonspecialist. Perhaps it is the suggestion that all one's life one has been tricked, all unaware, by the structure of language into a certain way of perceiving reality, with the implication that awareness of this trickery will enable one to see the world with fresh insight" (p. 27).

The hypothesis that the structure of the language one habitually uses influences the manner in which one understands his environment has thus far neither been proved nor disproved. However, that Whorf often substituted affection for objectivity in his analysis of Indian languages becomes apparent with the reading of almost any of the papers in this volume. Thus, statements such as those to the effect that the Hopi lan-

guage shows "a higher plane of thinking, a more rational analysis of situations, than our vaunted English" (p. 85), or "how much more precise and finely elaborated is the system of relationships in many such [preliterate] tongues" (p. 84) have made Whorf vulnerable to those critics who attack the lack of scientific rigor in his methodology. Too, his belief that "it was . . . enlightening to see English from the entirely new angle necessitated in order to translate it into Hopi" (p. 112) sounds suspiciously similar to the old classicist's superstition that everyone should study Latin, for there is no better way to learn English grammar. The two points of view are, perhaps, merely different sides of the same coin.

A more serious criticism of Whorf's methodology, pointed out in Carroll's very fair evaluation, is the undue importance given to translation as an index of the differences between languages. It is surprising, for example, that Whorf considered it so noteworthy that, say, Hopi had only one word where English has three; that there is no one-to-one correspondence between languages is obvious to anyone after his first experience with another language. Differences (or similarities) in the linguistic behavior associated with two events do not necessarily imply corresponding differences (or similarities) in the perception of those events. A number of factors may operate to cause such situations: metaphorical extensions, semantic change, idioms (that is, a case where the meaning of a complex whole cannot be predicted from the meanings of the constituent parts), and so forth. Would anyone, for example, infer that English speakers consider the two events of (i) *dying* and (ii) *hitting a pail with one's foot* as being closely related because they each may be referred to as "kicking the bucket?"

That Whorf's argument suffers from circularity of inferences has been pointed out by his critics. We must observe the linguistic and nonlinguistic events separately before they can be correlated; otherwise, the only evidence for differences in "world-view" turns out to be the linguistic differences. Even assuming that we find a striking difference in language structures, and what seems to be an associated difference in some nonlinguistic behavior, it must be demonstrated (i) how often such co-occurrences might be expected merely by chance, and (ii) what the exact nature of the relationship is. If A and B co-occur, either A causes B, or B causes A, or perhaps some other factor C causes both A and B. To use a trivial example of the relationships between language and culture, it is certainly likely that people who live near the sea and who are engaged primarily in fishing will have a large and precise terminology for things connected with fishing. However, no one would suggest that these people took up fishing because their language had such an appropriate terminology.

Carroll quite rightly points out that regardless of whether the linguistic relativity principle is valid or not, the interest it has aroused should not be allowed to minimize the attempts to search for language universals. Whorf maintains, for example, that such a contrast as that between verb and noun is "meaningless" in some languages—"in Nitinat it seems not to exist" (p. 99). However true this may be, it seems reasonable to assume that language as a form of learned behavior should be subject to the general laws that govern all learned behavior and that there should be some manifestations of these general laws in the forms of "universals."

These remarks are not meant to refute the Whorf hypothesis, but merely to indicate where some refinement is necessary before the hypothesis can be adequately tested. Collection of the data required for investigating the validity of the hypothesis will certainly be fruitful. It is hoped that the appearance of this book will further stimulate research in determining the nontrivial, other-than-chance relationships between language structure and nonlinguistic behavior, perception, cognition, and so forth—research of the nature Carroll himself is engaged in as director of the Southwest Project in Comparative Psycholinguistics.

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Scientific Books, Libraries and Collectors. A study of bibliography and the book trade in relation to science. John L. Thornton and R. I. J. Tully. Library Association, London, 1954. 288 pp. 24s.

In 1949, the senior author, librarian of St. Bartholomew's Hospital Medical College, London, issued *Medical Books, Libraries and Collectors*. He then promised a companion volume, *Scientific Books, Libraries and Collectors*. Now that this work has appeared, it should be welcomed by historians of science, librarians, collectors, and all who are concerned with the growth and development of scientific literature.

Although the book is mainly bibliographic in nature, it has the rare merit of being both readable and interesting. It is not, as the authors point out in their preface, "an exhaustive treatise on the bibliographical aspects of science, but rather an introductory history of the production, distribution and storage of scientific literature from the earliest times. Our aim has been the recording of information accessible only at the expense of much research, rather than the presentation of new material, and our selected bibliography guides readers to sources of additional information."

It may be said at once that the authors have succeeded very well in their aim. Although the volume appears to be small, a vast amount of information has been packed into it. There are 12 chapters that include such topics as scientific literature before the invention of printing, scientific incunabula, scientific books of the 16th century, 17th century scientific books, scientific books from 1700–1799, the rise of scientific societies, the growth of scientific periodical literature, scientific bibliographies and bibliographers, private scientific libraries, scientific publishing and bookselling, and scientific libraries of today.

In addition to many bibliographic footnotes, there is a valuable 26-page bibliography and an index.

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Science and Christian Belief. C. A. Coulson. University of North Carolina Press, Chapel Hill, 1955. 127 pp. $2.50.

Coulson is a professor of applied mathematics and an acknowledged authority in several fields of chemistry and physics. He is also a sincere Christian. Therefore, when he undertakes to talk about science and Christian belief, he deserves a respectful hearing from both scientists and churchmen. His book is one of the best discussions of this difficult subject that I have read.

The author's thesis is that science itself is essentially a religious activity. In science, discovery of facts alone is not enough. Facts are all related to each other, and what the scientist especially seeks is to tie them together by unifying concepts. "Scientific truth means coherence in a pattern which is recognized as meaningful." Religion, in the same way, seeks unifying concepts among its own facts and experiences. Both share a common ignorance and a common hope. Furthermore, the underlying assumptions of science are essentially moral and spiritual ones—honesty, integrity, humility, hope, enthusiasm, patience, cooperation with others, and the use of both reason and imagination. Both seek for truth and for order and constancy in nature, but they study different aspects of the truth as one might study different aspects of the complex blueprints for a building. An artist would see in these blueprints something

quite different from what an engineer would find.

It is, therefore, harmful to separate sharply the scientific approach from the religious approach to truth. "Religion is the total response of man to all his environment." The author stresses the importance of "reflection," an attempt to build a full three-dimensional framework of all the reality we encounter in our various experiences. Such reflection will show, he believes, that these experiences provide a conviction of revelation in both science and religion, a sense of unity in nature, and a quality in this unity that is essentially spiritual. This personal and spiritual quality reaches its highest manifestation in Jesus Christ.

Coulson's plea that religion should not be divorced from science but should interpret all our experiences will doubtless meet with much support. It is well, however, for us to remember something which, in my opinion, he does not stress sufficiently—that the approach of science to reality is chiefly through the intellect, while the approach of religion is chiefly through spiritual insight. It is the unwillingness of many men of science to recognize the validity of the latter as an avenue to truth that has been the main cause of the so-called "conflict" between science and religion.

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Ludwig Boltzmann, Mensch, Physiker, Philosoph. Engelbert Broda. Deuticke, Vienna, 1955. viii + 152 pp.

It is 50 years since the tragic end of Boltzmann, one of the greatest physicists of the last century and the founder of the modern theory of statistical physics, but this is the first full-length biography of him. Written by the radiochemist, Broda, the biography is introduced by Hans Thirring, who briefly gives the argument of Boltzmann's explaining heat processes mechanically. The book is divided into four sections.

The section, "Boltzmann the man," not only describes his academic career, which was many sided—he was, in succession, professor of mathematics, of experimental physics, of theoretical physics, and finally of philosophy—but is particularly valuable because, like the whole book, it contains copious quotations from Boltzmann's own writings and statements by former students and friends, and thus it gives a vivid picture of this unique personality.

The second section. "Boltzmann the physicist," is, in my opinion, too brief. However, it gives all the most important discoveries, stressing Boltzmann's outlook on atomistics and mechanics and in particular showing that Boltzmann not only had developed kinetic theory but had also used the kinetic picture in the theory of matter, and in electricity, where he first gave the atomistic explanation for the Hall effect. It was Lorentz who said, "to Boltzmann goes the honor of first having estimated the velocity of electricity in solids."

Boltzmann's most important contribution was the formulation of the H-theorem that leads to the relation between entropy and probability, which according to Planck is written in the simple equation, $S = k \ln P$, where S is the entropy, k the Boltzmann constant, and P the probability. It is this equation that has been put on Boltzmann's tombstone. To this generation, Boltzmann is best known for these theoretical investigations. E. Mach called him an experimenter of unsurpassed technique, and it was Boltzmann who investigated the dielectric constant and its relation to the refractive index (Maxwell relation), who measured the force on a dielectric sphere in an electric field, and who also determined the wavelength of Hertzian waves (microwaves) with an interferometer.

The third section is "Boltzmann the philosopher." Boltzmann was unfortunate in that most of his contemporaries, particularly the natural philosophers as represented by Ostwald and Mach, did not believe in the reality of atoms. His polemic arguments against their criticism took a great deal of his time and to a certain extent colors his philosophical lectures, which are discussed in some detail in this book.

Boltzmann was of an artistic and sensitive nature but full of humor. Californians then and now will be delighted with his statement in one of the *Popular Lectures*, "The trip of a German professor to Eldorado." In describing the trip to California, he calls it something "quite exquisite" and says, "A trip to California is Champagne Veuve Cliquot and oysters." Not many physicists will say what he says in the preface to his *Popular Lectures*, which he dedicated to the memory of the great German poet, Friedrich Schiller. He said, "This dedication is no empty phrase. Through Schiller I am what I am at the present time. Without him a man with the same type of beard or nose could exist, but never myself. There is only a second one who had a similar influence on me and that is Beethoven."

It is characteristic for this outlook that in his *Popular Lectures* the names of Beethoven, Schiller, Mozart and some other artists appear frequently and that he is full of the enthusiasm of an artist who is taken with the beauty of the physical theory in very much the same way as Einstein himself, who was profoundly influenced by Boltzmann's writing.

The final section summarizes briefly Boltzmann's importance for modern physics. The book is well worth reading as an introduction both to Boltzmann's personality and to his outlook on physical theories and philosophy.

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Physical Sciences

Relativity: the Special Theory. J. L. Synge. North Holland, Amsterdam; Interscience, New York, 1956. xvi + 450 pp. $10.50.

J. L. Synge of the Dublin Institute for Advanced Studies is well known as a competent mathematician who is also an excellent writer of texts on an intermediate level. Readers of his mechanics text will find the present book on relativity on a substantially more demanding level, but written with the charm that makes reading a pleasure and that is a substantial help over the rough spots.

The subject matter of this book covers the geometry of the Minkowski manifold perhaps more comprehensively than would be necessary for a presentation of of the special theory alone. This excess mathematical baggage is, however, well justified for anyone who contemplates going on at some later date to a study of the general theory of relativity. Because Synge is by inclination a mathematician rather than a physicist, he begins not with a discussion of the physical reasons leading to the replacement of the Newtonian by the Minkowskian continuum but rather with a semipostulational discussion of the logical possibilities. As the discussion progresses, physical and observational arguments are given their due attention as well. The discussion of the mathematical and the kinetic aspects of the Minkowski space and the Lorentz transformations occupies roughly the first third of the book.

Next the author proceeds to what is usually called relativistic mechanics. It is well known that relativistic mechanics cannot properly cover as much ground as does Newtonian mechanics, because of the incompatibility of the Lorentz transformation with action-at-a-distance. Relativistic mechanics concerns itself, accordingly, with action at infinitesimal distances—that is, with collisions between mass points and with the laws of fluid dynamics. There is no attempt to cover relativistic quantum mechanics (that is, the theory of a single Dirac particle). The Compton effect is treated (as it can be done very adequately) as the collision between a classical point electron and a classical photon—that is, a particle of zero rest mass. Synge even treats pair creation and pair annihilation purely classically. It is comforting to see how much information can be obtained even in this simplified approach. Mechanics comprises the second third of the book.

Relativistic electrodynamics, including the classical theory of radiation interaction, represents the concluding portion of Synge's book. It is perhaps here that one misses most some discussion of recent work in quantum theory. It is precisely because of the lucidity of the discussion that one would like to see it extended further. Altogether the book deserves high praise.

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Electronic Transformers and Circuits. Reuben Lee. Wiley, New York; Chapman and Hall, London, ed. 2, 1955. xvi + 360 pp. Illus. $7.50.

The stated objective of Lee's book is to provide a reference book on the design of transformers for electronic apparatus and to furnish electronic equipment engineers with an understanding of the effects of transformer characteristics on electronic circuits. To this end, chapters are included on transformer construction, rectifier performance, audio transformers and circuits, higher frequency transformers, control transformers, magnetic amplifiers, and pulse and video transformers and circuits. A quite complete bibliography is included, and the book is well indexed.

As stated in the preface, in a book of general coverage there is room for only a brief treatment of any phase of the subject. In general those phases dealing with low-frequency transformer design are quite adequately covered; however, the reader will find comparatively little in this book to aid him in the design of high-frequency transformers. Probably the most thorough portions of the book are those dealing with comparatively low-power transformers of conventional design—for example, filament transformers, anode transformers, combination filament and anode transformers, and reactors; with audio transformers; and with pulse and video transformers. The material on magnetic amplifiers forms a very good summary of this extremely important field, and sufficient theory is presented to give the reader an adequate background.

The text is well interspersed with charts, graphs, and design guides, and there are numerous examples to illustrate the design procedures. Despite this, however, the reader will still find it necessary to call on the benefits of experience to indicate, for example, allowable values of core induction, wire current density, estimated losses, regulation, and so forth. Useful formulas and graphs are provided from which such design parameters as open-circuit and leakage inductances and interwinding and total capacitances may be calculated. Thus the treatment of audio transformer design is well handled in the amount of space that is provided, and it should provide the reader with the essential details for the design of conventional audio amplifiers.

The approximate treatment of the transient response in pulse and video transformers should also provide good reference material. The treatment of radar pulsers and charging reactors is adequate. Sweep circuit operation is covered in some detail, although there is comparatively little on transformer design for this application. Ferrites, which find particular application in this and in high-frequency transformer design, are given very little mention. There is a brief mention of this extremely important class of materials on page 33, and there are two pages on high-frequency uses on pages 217–219, and only passing references elsewhere.

High-frequency transformers—for example, those used in intermediate-frequency amplifiers, are treated rather lightly on pages 224–232, and this treatment deals exclusively with the calculation of inductances, there being no information on the coil Q. Readers will find this information useful only as background material; it is insufficient for the design of such transformers.

Another omission, and a rather serious one, is the lack of adequate treatment of audio transformers for use with transistors. This important field of application is mentioned briefly on one page, and comparisons are drawn with conventional tube circuits that are misleading, if not downright incorrect. Various factors—that the impedance levels of transistors are radically different from those of electron tubes, that direct current usually flows in both windings, that small size is even more necessary here than with tubes, and that transistors are bilateral devices—would make a more detailed treatment of the subject desirable.

This book has valuable reference material for the design of low-frequency transformers and reactors. The high-frequency treatment is useful for background but inadequate for design purposes.

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Principles and Problems in Energetics. J. N. Brønsted. Translated by R. P. Bell. Interscience, New York, 1955. vii + 119 pp. Illus. $3.50.

The student of thermodynamics quickly recognizes that the subject is complete and exact. If he accepts the laws of thermodynamics, elaboration of the subject is accomplished by relatively trivial mathematical manipulations. The final results thus require no proof, either theoretically or experimentally, and the difficulties that arise for the thoughtful student of thermodynamics are concerned almost entirely with the first and second laws.

J. N. Brønsted, about 20 years ago, became disturbed over the classical formulation of the laws of thermodynamics. Over the next 10 years he developed what he preferred to call a system of energetics. He believed that it was simpler than traditional thermodynamics, and he hoped that it could be used for irreversible processes. The methods that he devised for these purposes cannot be described briefly. It can be said, however, that the first law of thermodynamics becomes a work principle, which applies to reversible processes, and that the second law is broadened to a heat and equivalence principle, which includes irreversible processes. It can also be said that Brønsted never loses the reader in mathematical complexities and that he always emphasizes the physical concepts.

The results of his deliberations appeared usually in Danish journals; this collected account of them was first published in Danish in 1946. Since English-speaking scientists are usually poor linguists, we should be grateful for an opportunity to learn about energetics in a readable translation by R. P. Bell. An interesting foreword has been added by V. K. La Mer, who has enthusiastically introduced the subject to his own students. Anyone who is seriously interested in thermodynamics should read this book.

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Mesons and Fields. vol. I, *Fields*. Silvan S. Schweber, Hans A. Bethe, and Frederic De Hoffmann. Row, Peterson, Evanston, Ill., 1955. 449 pp. Illus.

This is logically the first part, if chronologically the second part, of a two-volume work under the above title. Volume II, *Mesons*, by Bethe and De Hoffmann, has already appeared; the present volume is primarily the work of Silvan S. Schweber. It provides an excellent introduction to present-day field theory and the associated covariant perturbation formalism. The background required is approximately a standard course in quantum mechanics, making the volume well suited for use as the basis of a graduate physics course, which, in fact, was its origin.

The book first develops the equations and formal properties of relativistic particles with spins 0 to ½, and then pre-

sents an exposition of Feynman's positron theory for the elementary case without second quantization. The text derives coherence from the emphasis throughout on Feynman's procedures, for which pedagogical advantages of "visualization and ease of understanding" are claimed. Part II of this volume introduces standard field theory, including all types of coupling necessary for electromagnetic and mesonic interactions. The fields are next quantized, covariant perturbation theory is introduced, and its use in calculating the S matrix is discussed. The equivalence of the whole scheme with Feynman's techniques is established, following the procedures of Dyson and Wick. Almost the last third of the book is devoted to the renormalization of divergences, which forms so important a part of contemporary field theory. Concluding sections discuss recent developments.

There are a number of nice features in this book, such as the use of radiative corrections to Coulomb scattering as a means of calculating the Lamb shift and the magnetic moment correction $\Delta\mu/\mu = \alpha/2\pi$. The Bethe-Salpeter two-body equation is introduced early in an elementary way and then rederived in more detail in a concluding section. The style of the text is extremely readable and its freedom from typographical errors is remarkable. The chief omission seems to be in applications of the formalism to meson-nuclear problems: for instance, the Tamm-Dancoff method is not mentioned, nor is isotopic spin—all examples are worked out for neutral mesons. Of course these matters are taken up in volume II, but their lack makes volume I a little less self-contained. Although volume I may be of less widespread and immediate interest than volume II because of its formal character, by the same token, it is likely to be the more enduring member of the pair.

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An Introduction to Reactor Physics. D. J. Littler and J. F. Raffle. McGraw-Hill, New York; Pergamon, London, 1955. viii + 196 pp. Illus. $4.50.

This work is based on a set of introductory lectures that were delivered by the authors to a mixed audience of engineers and physicists who were involved in the design and construction of nuclear reactors. The book deals exclusively with reactors in which fissions are produced predominantly by thermal neutrons.

The first four chapters are introductory in nature. They deal with the radioactivity decay laws, nuclear binding energies, the organization of the periodic table, and other background information that is not directly applicable to the design of reactors. The kinetic theory of neutrons is presented in Chapters 5, 6, and 7. Topics treated include neutron nuclear interactions, the concept of the cross section, the diffusion equation, and general remarks on chain reactions. Chapter 8 deals with the critical conditions for unreflected, homogeneous reactors. Heterogeneous reactors are discussed in Chapters 9 and 10. The time-dependent behavior of reactors is discussed briefly in Chapter 11. Chapter 12 deals with a collection of miscellaneous problems related to the running of a reactor at power. These include the temperature coefficient of reactivity, the absorption of neutrons by fission products, the activity of fission products as a function of time after shutdown, and the radioactivity of the air in an air-cooled reactor. In Chapter 13, shielding against gamma rays is discussed. Chapters 14 and 16 treat radiation detectors and experimental methods for measuring reactor parameters. Radiation damage is the subject of Chapter 15.

The background material in the first four chapters is condensed in a terse fashion. For this reason, some of the examples require greater explanation. This terseness is also apparent in the section on neutron interactions where, for example, the inverse velocity law is mentioned only casually.

The behavior of thermal neutrons in lattice reactors is treated better than any other subject in the book. The treatment is of a handbook type with sufficient explanation, formulas, and examples to instruct the reader in the techniques of lattice calculations.

In general, the treatment of the subject matter is uneven. Undue attention is given to mathematical details in some places, while the physics is glossed over. The mathematical solution of the diffusion equation is discussed in detail, while hardly a mention is made of "age" theory. Derivations are carried part way with vigor and precision and completed by use of formulas whose origin is obscure.

It is not clear for whom the book is intended. The material is not sophisticated enough for the serious student, and yet it is not useful as a handbook for the reactor designer.

The shielding portion of the book discusses the various processes by which gamma rays are attenuated. The energy dependence of compton, photoelectric, and pair-production cross sections is given, but there is no mention of build-up factors. Principles of neutron shielding are not discussed at all.

The language in many cases is less precise than one would like. The assumptions underlying the derivations are not always stated. In addition, typographical errors, especially those in formulas, represent serious pitfalls for the reader.

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Principles of Guided Missile Design. *Guidance.* Arthur S. Locke in collaboration with Charles H. Dodge, Samuel F. George, Laurence F. Gilchrist, William C. Hodgson, John E. Meade, John A. Sanderson, and Charles F. White. Van Nostrand, New York, 1955. xvii + 729 pp. Illus. $12.50.

This book, which is the first of a series in the field of guided missile design, is intended for the serious graduate student as well as for workers in the field. The complete series, which will include, among others, topics on propulsion, aerodynamics, launching, testing, and operations research, is edited by Grayson Merrill. A. S. Locke, who wrote most of the volume on *Guidance*, was greatly assisted by seven principal collaborators, each of whom is an expert in a given field.

This book represents the first serious attempt to collate the voluminous declassified literature in the various areas that collectively form the field of missile guidance. This Herculean task has been performed quite well by the author and his collaborators. It is not easy reading, nor is this its intent. The symbolism for the mathematics varies considerably through the volume, but each chapter is essentially self-contained in that the notations used are, in general, sufficiently well defined to avoid confusion.

Some of the chapters go into great detail on certain subjects—for example, "Transmission of radio waves," in which most of the material is not new or even presented in a better form than may be found in standard texts on this subject. Other chapters, which discuss subjects that are bound to play an increasingly important role in the future of missile guidance, are abbreviated or only present elementary principles. These omissions are entirely excusable on security grounds. However, the chapter on the measurement of missile motion, although satisfactory as an elementary presentation of the problem, could have been expanded considerably without a violation of security regulations. The fundamental laws that govern the action of gyros and their associated parts are well known and have been recorded in great detail in texts and other publications in countries abroad. References to these

works are not even included at the end of this chapter. In general, in this area, too few references are given to other works on the various subjects in most of the chapters, and yet, in many ways, the volume is not self-sufficient.

The book, however, contains many very practical hints and suggestions that were obviously born out of the experiences of the author and his collaborators. The chapters on "Target considerations" and "Prelaunching and launching" are particularly significant in this regard. The book also gives many examples of economic considerations, such as Chapters 15, 19, and 20, all of which are so necessary to guide our young and brilliant engineers in this highly important field.

In general, the book provides a valuable aid for advanced courses in universities, either as source material or as an advanced part-time text in specialized courses. Its primary value, however, is that it provides source material and refresher material to engineers who must perform daily tasks as advanced systems designers in both civilian and military establishments in this highly complicated field.

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Astrophysical Quantities. C. W. Allen. Athlone Press, London, 1955 (distributed in the United States by John de Graff, Inc., New York 10). xi + 263 pp. $10.

Here is a volume that every teacher and research worker in astronomy and its allied sciences will want handy for quick reference. A large number of physical constants and quantities are tabulated, in addition to what the author considers to be the best available data on the earth's atmosphere and interior, the sun, stars, planets, nebulae, and galaxies. Great care has been taken to give clearly the units of every quantity that is tabulated, and numerous conversion formulas are included. Copious references provide the reader with the sources from which much of the information is drawn. The judgments by which the author derived the tabulated values are not given.

The material embraced in this compilation runs over such a wide range of subject matter that it is scarcely to be expected that all topics would be covered to the same degree of thoroughness and reliability. More data pertaining to radio astronomy might have been included advantageously. In a number of instances, better data than those quoted exist in the literature. The temperatures quoted for the interior of the earth seem improbable. The failure to distinguish between the Rosseland and Chandrasekhar mean absorption coefficients in section 65 is unfortunate. Section 95 does not do justice to the various three-color systems such as those of Becker, Johnson, and Morgan, which appear to be of increasing importance in photometric work. The table of line emissions in planetary nebulae would have been much better if it had contained the actual data for some particular nebulae. The table of partition functions is somewhat inadequate.

Various minor shortcomings are manyfold offset by the great service that has been rendered in putting together in a concise fashion so much of the basic useful quantitative data in astrophysics.

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Principles of Physical Metallurgy. Morton C. Smith. Harper, New York, 1956. 417 pp. Illus. $6.

This book is the first of a two-volume series. The second book, *Alloy Series in Physical Metallurgy* was not available for this review. The author's expressed purpose is to ". . . assist the thoughtful metallurgist . . . in achieving a fundamental understanding of metal behavior." Smith recognizes the difficulty in pleasing both the scientist and the engineer in one book, but, unfortunately, it appears that his efforts please neither one.

A little less than the first half of the book is devoted to elementary atomic physics, crystallography, crystal types, polymorphism, and crystal imperfections. This last subject includes such diverse topics as freezing of metals, dislocations, Hume-Rothery rules, and ordering. A brief chapter on magnetic and electrical properties completes the first section. The organization and method of presentation are neither bad nor distinguished; in essence, the author has boiled down the well-known books of Seitz, Cottrell, and Barrett. The coverage of topics is broad rather than deep, and tends to vary in emphasis. Four pages are devoted to the amorphous "Beilby layer," and somewhat less space is given to dislocations, although the latter are subsequently reintroduced. Dislocations, lineage, and mosaic structure are treated successively with no attempt at interrelation.

The greater part of the book concerns deformation of metals, effects of deformation and temperature, and fracture. The subjects are treated in a rather loosely knit fashion. Descriptions of the various properties are usually general with occasional references to specific alloy systems. Little use is made of the theories developed in the first half of the book. Although about 20 pages are devoted to creep, experimental work and theory of the last 10 years are largely ignored.

A great many (apparent) statements of fact are open to question; for instance, Smith states that second-stage and third-stage creep do not occur in compression, although it is well known that they do. In another example, he states that mechanical polishing of copper results in mechanical twinning. Unfortunately, there are very few references, so that one cannot trace some of the author's unusual conclusions to their sources.

Attempting to educate the engineering metallurgist, as the author intends, is a worthy and much-needed endeavor, but in a book of this size it is difficult to give more than superficial coverage of many important topics; the author has done no more. Furthermore, the lack of references and the use of books for most references (thereby causing much material to be out of date) decrease the usefulness of the book if it should whet the interest of the reader.

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Structural Geology for Petroleum Geologists. William L. Russell. McGraw-Hill, New York, 1955. x + 427 pp. Illus. $7.50.

Russell's new textbook on *Structural Geology for Petroleum Geologists* has been prepared with an objective that, to my knowledge, is new, original, and so worthy that I wonder why it had not been thought of before. As the title indicates, it is neither a textbook on petroleum geology nor a textbook on structural geology, although it includes and combines subject matter ordinarily treated in each of these, omitting parts of the other subjects that do not bear on the new objective.

In the subject of petroleum geology, it thus omits such matters as chemistry of hydrocarbons, the hydrology of petroleum deposits, and the origin, migration, and accumulation of petroleum; in the subject of structural geology, it omits structural features of metamorphic and igneous rocks, neither of which ordinarily contains petroleum deposits. The text is, however, not merely a treatment of the structure of known oil and gas reservoirs, for the petroleum geologist is less a developer of proved deposits than he is an explorer for deposits yet un-

known and undeveloped. His field is therefore that of regional geology and all sedimentary rocks. Thus, a text on structural geology for petroleum geologists must also be a text on the structural geology of sedimentary rocks in general.

This viewpoint is indicated by the subject matter of the chapters of the present book, which includes treatment of folds, faults, joints and fractures, unconformities, salt domes, buried hills, sedimentary features, regional structures, superficial structures, and continental shelves. The book concludes, however, with two chapters of more practical application to petroleum geology on classification of traps for oil and gas accumulation and on structural factors in petroleum prospecting.

With these objectives, the book may well find its place as a textbook in a structural geology course in those schools of the country where the surrounding terrain is made up dominantly of sedimentary rocks, or where the study of geology is primarily in preparation for entrance of the student into the various professions of the petroleum industry.

In assemblying his book, Russell has covered a vast amount of original source material, largely contained in articles in the technical journals, and in the various state and federal publication series. One of the most useful feaures of the book for the professional geologist, in fact, is the careful documentation of most topics, both in footnote references and in the lists of "additional references" at the ends of the chapters. The professional geologist will find these citations an up-to-date survey of numerous topics of interest.

In a work with objectives as novel as this, a first attempt may not prove to be as well rounded as later ones, by the same author or others; in the present book, I feel that this is to some extent the case. Thus, some of the chapters seem to be largely revisions of chapters in the author's previous *Principles of Petroleum Geology*; they cover essentially the same subject matter with only slight change in emphasis. This is notably true of the chapters on salt domes in the two books, and to a lesser extent of the chapters on sedimentary structures, structure of the continental shelves, and classification of oil and gas reservoirs; the two books, instead of being mutually supplementary, partly duplicate each other.

Also, in this first attempt, treatment of the subject matter appears uneven. Some chapters, such as those on faults and sedimentary structures, are largely statements of definitions and general principles, little fortified by examples or indications of how the principles might apply to petroleum geology. On the other hand, some topics, such as salt domes and buried hills, are rather exhaustively covered and well supported by examples.

Some areas with special problems are also given an interesting summary treatment—for example, the problem of the cherts of the Ouachita facies (pp. 268–270)—and I wish that more of these summaries could have been included. I would like to see a treatment in one place of the assemblage of related structural features that characterize the Gulf Coastal Plain, to which some geologists have given the term "homoclinal tectonics"; many of the individual features are mentioned throughout the text, but their relationships to each other in bringing about a peculiar structural style in the region is not made evident. The chapter on faults contains little mention of faults with large components of transcurrent movement of the type of the San Andreas fault in California, although some of them, such as the San Gabriel fault north of Los Angeles, pass through productive oil territory. It is also disappointing to find no mention of the problems of "gravity tectonics" in the chapter on superficial structures, although structures resulting from this process are important in the oil fields of Iran and northwestern Peru, and the area where they are most spectacularly developed, in the northern Apennines of Italy, is currently being prospected for petroleum. Some of these features may seem odd or exceptional, yet they may be more common than realized, and the petroleum geologist should make himself aware of them.

On the other hand, I am puzzled at the inclusion, and in some cases the rather full discussion, of some items that seem to have little bearing on petroleum geology, such as erratic blocks (pp. 287–288) and cryptovolcanic structures (pp. 329–332).

It seems to me, however, that the greatest opportunity for improvement of the book is in the text figures. Many of these are simply outline sketches illustrating general principles such as an instructor customarily puts on the blackboard to demonstrate some item to his classs. A printed book, in contrast to an oral lecture, provides an opportunity for more elaborate demonstration from actual examples. Such demonstration would appear to be desirable, for most of the students who will use the book will have little notion of how the principles illustrated in the sketches apply in field problems or in the occurrence of petroleum. Even some of the general discussions in the text might well have been tied to cross-sections or maps showing specific structures, rather than to theoretical diagrams.

For example, under faulting, reverse drag shown in Figs. 5–1 and 5–12 might have been illustrated by a cross-section of a fault with reverse drag in the Coastal Plain area; thrust faults shown in Figs. 5–9 and 5–13 could have been illustrated by sections in the Ventura Basin, Calif., the Anadarko Basin, Okla., and Turner Valley, Alberta; and fensters in a thrust fault shown in Fig. 5–14 could have been illustrated by a section of the Rose Hill oil field, Va. One misses, too, maps showing the interesting and significant relation between faults and oil-bearing anticlines, such as those in the Kettleman Hills field, Calif., and various anticlinal fields of Wyoming.

Examples of this sort are in proved oil territory; many of them involve producing oil fields; all are supported by abundant drilling data that indicate the structure for many thousands of feet below the surface. They would emphasize to the student that the structures discussed in the text are not merely theoretical concepts of whose application he is uncertain, but genuine features of the sort with which he will have to deal in his work as a professional petroleum geologist. It is true, of course, that the examples are in developed territory, whereas much of the student's professional work will be as an explorer in areas where these have not been deciphered and have not been proved by drilling, but they will demonstrate to him the types of structure he must consider in planning for the drilling of promising localities.

In summary, in these days when efforts are so often duplicated by different authors, so that the purchaser of technical books is more often puzzled to make a choice between several works on the same subject, rather than to locate the unique book for his purpose, Russell is to be congratulated for discovering a well-known subject on which no specific book has been prepared. Opinions will probably differ from one reader to another about how fully the objectives of the book have been met, but I feel that this worthy undertaking could be further amplified and improved. This may well be Russell's intention as he continues to gather material on the subject and to teach it to his classes. In the meantime, the book as now published is a useful and much needed source book on an important and neglected subject.

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New Zealand Geomorphology. Reprint of selected papers 1912–1925. C. A. Cotton. New Zealand University Press, Wellington, 1955. 281 pp. Illus. + plates. 42s.

Those who are familiar with the students of geomorphology need no introduction to C. A. Cotton. Indeed, all the older students have already read these chapters in the various learned journals,

for if we inspect the subtitle, we find "Reprints of selected papers 1912–1925." And I would recommend that all who have not read them do so. First, they describe various phases of the geomorphology of an area that few of us are fortunate enough to see. Second, they are the building up of the ideas that Cotton's students have carried beyond the borders of New Zealand and applied in other regions with considerable ability. Third, they are good reading.

Sixteen papers are reprinted, and I can see no good being served in listing the 16 titles with their sources. I compliment the Victoria University College in bringing together these 16 excellent papers in one volume and wonder if it was a birthday surprise for Cotton for his 70th year. I am sorry, though, that the original paging is lost.

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Physical Chemistry. Farrington Daniels and Robert A. Alberty. Wiley, New York; Chapman & Hall, London, 1955. vii + 671 pp. Illus. $6.50.

This new textbook is a successor to the long series of previous editions that were initiated by Frederick H. Getman in 1913 and continued by Farrington Daniels. Some new subject matter and new exercises and problems are introduced, and, in some instances, the order in which topics within a chapter are presented has been changed. The sequence and titles of the 20 chapters are essentially the same as in the last edition of Daniels' book. The text follows its predecessors' excellent example in that it has a number of exercises throughout each chapter that illustrate the more important principles in detail. References to original work and to more recent reference works have been brought up to date and many new citations are included.

Although adding new figures to the chapter on crystals, for instance a Fourier plot for maleic acid, is highly desirable, it apparently necessitated omitting other figures that were quite useful for class purposes, such as those illustrating interplanar distances. The addition of some figures and some rearrangement of topics, such as placing Onsager's conductance theory in the chapter on electrolytic conductance rather than in the chapter on ionic equilibria, make the teaching of the subject matter of these chapters somewhat more orderly. A brief discussion of the application of light scattering and the more extended discussions of diffusion and other physicochemical principles in the chapter on colloids are significant improvements.

The inclusion of the chemical potential and free-energy functions is a welcome addition, although use of the former may well have been illustrated in connection with the chapters on equilibrium. Tables of heats of formation, bond energies, standard entropies, standard electrode potentials, and so forth, have been revised, using the most recent values. The contraction effected in the appendix of this new textbook is more apparent than real since the essentials of much of the material deleted from the appendix of previous editions is incorporated in appropriate chapters. The omission of elementary subject matter that was included in earlier editions seems quite appropriate for, as the authors say, such topics are now generally handled adequately in prerequisite courses.

The fundamentals of physical chemistry are well covered, and experience with teaching this book's forerunner indicates that a radical departure in the new volume from the established pattern would hardly be justified at this time.

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Chemical Engineering. vol. 2, *Unit Operations.* J. M. Coulson and J. F. Richardson. McGraw-Hill, New York; Pergamon, London, 1955. 975 pp. Illus. $9.

Of all the engineering disciplines, chemical engineering defies attempts at systematization. The range of the field is so great that the sort of continuity one expects in moving from one topic to another is impossible. These reasons, perhaps more than any others, explain the sort of treatment of chemical engineering given in U.S. schools under the catchall "unit operations." Now come two Englishmen who recognize that unit operations is exactly chemical engineering if we include a discussion of basic theory, as was done in volume I. One must admit that basic theory is a strongly unifying ingredient.

Coulson and Richardson have done a remarkably fine piece of work. Their first volume is superior to anything that has thus far appeared in the United States, and now the second volume presumably integrates the field. Unfortunately, this cannot be so. There must eventually be a third volume, because the discussions on the diffusional processes are not only sketchy but also are not sufficiently comprehensive.

Since the last chapter of volume I was devoted to mass transfer theory (with a single application), one had begun to look forward to what has long been needed in chemical engineering—a detailed analysis of the diffusional processes. This has not been done to the extent anticipated. Instead, a great deal has been written with reference to particulate technology—particle motion, sedimentation and fluidization, size reduction and classification. All these are, of course, needed and are better and more thoroughly done in the second volume than in several U.S. textbooks combined. But some day, a third volume must appear that will bring together the sections dealing with the diffusional processes. Then Coulson and Richardson will have settled once and for all what chemical engineering means and is.

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Biochemistry and Physiology

Biochemistry and the Central Nervous System. Henry McIlwain. Little, Brown, Boston, 1955. vii + 272 pp. Illus. $9.50.

This book is apparently intended for use as a textbook of neurophysiology and psychological medicine and, as such, it fulfills a valuable function in bringing students and workers in the general area of CNS research up to date on biochemical methods and findings in the field. Although in general, the author deals with the same material as is covered by the recent encyclopedic works on neurochemistry by Elliott *et al.* and by Waelsch, his treatment is necessarily far more concise but still sufficiently detailed for clarity. Many of the chapters contain well-documented, valuable summaries of quantitative data collected from various sources.

The chapter headings are "Biochemical studies of the brain," "Metabolism of the brain *in situ*," "The chemical composition of the brain," "Metabolism of separated cerebral tissues," "Cell-free cerebral systems," "Glycolysis and an oxidative pathway," "Pyruvate metabolism," "Oxidative phosphorylation," "Amino acids and cerebral activities," "Vitamins and the central nervous system," "Cerebral lipids," "Cytochemical and histochemical aspects," "Chemical and enzymic make-up of the brain during development," "Acetylcholine, sympathin and related substances," "Depressants and excitants of the central nervous system," "The speed of chemical change in the brain."

It seemed to me that the discussions of general metabolic processes in the brain—for example, glycolysis and the tricarboxylic acid cycle—although quite adequate themselves, did not emphasize sufficiently that these, or closely related reactions, occur in other mammalian organs as well as in some microorganisms and plants. This suggests that the brain is not distinguished from the other organs on the basis of its gross energy-

yielding or synthetic reactions, but that these reactions supply the over-all potential for the very rapid ion-transport events that are probably responsible for its characteristic electric activity.

For this reason, perhaps, the interested reader is bound to feel some disappointment on reading any book of this type. It seems that, as yet, research at this level falls short of a biochemical "explanation" for psychological, or even neurophysiological, phenomena.

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Androgens. Biochemistry, physiology, and clinical significance. Ralph I. Dorfman and Reginald A. Shipley. Wiley, New York; Chapman and Hall, London, 1956. 590 pp. Illus. $13.50.

Experimental endocrinologists, biochemists, and clinicians will enjoy this reference volume as a source book of information on the androgens. There are 1852 references in this medium-sized book, which consists of four parts: (i) "Introduction," (ii) "Biochemistry," (iii) "Physiology," and (iv) "Clinical aspects."

The arrangement of the textual material is clear, concise and presented in an interesting manner. The four sections are further subdivided into 20 chapters: (i) "General aspects" and "Historical background"; (ii) "Sources of androgens," "Isolation and chemistry of androgens and related compounds," "Preparation of urinary extracts," "Assay of androgens and 17-ketosteroids," "Metabolism of the androgens," "Relative activities of androgens," and "Biological actions and interactions of androgens"; (iii) "Androgens and the embryo, intersexuality," "Actions of androgens on sex structures of animals," "Androgens and behavior," "Influence on endocrine glands, other than gonads, and various non-endocrine glands," and "Influence of androgens on metabolism and enzymes"; (iv) "Normal puberal development in boys," "Conditions of androgen excess," "Androgen deficiency—hypogonadism," "Androgen therapy," "Androgen preparations and methods of administration," and "The excretion of androgens and 17-ketosteroids in various clinical conditions."

Immediately following a very comprehensive review of the clinical aspects of the androgens is an appendix containing "Names and structural formulae of compounds," "Preparation of urinary extracts for 17-ketosteroid and androgen assay," "Androgen bioassay methods," and "Chemical assay methods."

It is apparent that the authors have an excellent appreciation of the literature on this subject, and their survey of the basic and clinical literature, collated in an interesting fashion, should prove useful to the busy investigator. The combination of a basic scientist and a clinician adds considerable merit to their unbiased treatment of the subject matter.

This reference book is so heavily laden with information that the two unqualified statements I observed should be cited only for academic reasons devoid of any tones of criticism. On page 4 it is stated, ". . . the adrenocorticotropic hormone (ACTH) is essential for the full function of the adrenal cortex . . ." Robert Gaunt [*J. Clin. Endocrinol. and Metabolism* **15**, 621 (1955)] states that "Although the mechanisms which regulate the secretion of aldosterone are not yet known, one important fact is all but certain: aldosterone is not under the complete and direct control of ACTH, as are the other major corticoids." Therefore, according to Gaunt, it seems inadvisable to say that "ACTH is essential for the full function of the adrenal cortex." Further evidence bearing on this point may be found in Gaunt's paper. On page 16 the authors refer to a "female prostate." The term is incorrect, and it is believed that they are referring to the urethral glands including paraurethral ducts, a structure in the female that is homologous to the male prostate [H. Morris, *Human Anatomy*, J. P. Schaeffer, Ed. (Blakiston, New York, ed. 11, 1953), pp. 1565, 1567–1568].

In the final analysis, it is my opinion that the authors should be congratulated in bringing 1852 references together on the androgens. They have used these references well in writing their volume. This book could be greatly improved by the inclusion of more photographic material and by the citation of reviews, both of which could serve to highlight information that is at present documented elsewhere.

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Immunology and Serology. Philip L. Carpenter. Saunders, Philadelphia–London, 1956. vi + 351 pp. Illus. $6.50.

This is the latest addition to a field in which, as the author points out, there is a shortage of textbooks. It will be the more welcome because knowledge of the subject is growing rapidly, and numerous significant advances have been made not only since the publication of the second edition of Boyd's book in 1947, but also since the publication of Raffel's book in 1953.

The book has many other virtues. For one thing, it is smaller than either of the other two books, and thus perhaps better adapted to the needs of students in the invariably abbreviated courses in immunology that are available in our medical schools. It shows throughout the signs of having been written to accompany a course that emphasizes laboratory serology. Among the attractive features are a clear discussion of the physical properties of the serum proteins that is liberally illustrated with electrophoretic and ultracentrifugal diagrams, a discussion of plasma fractionation, good accounts of the current theories of antibody formation, a rather full account of the phylogenetic applications of the precipitation reaction, and a good account of complement fixation.

The subjects that must be discussed in such a book are pretty generally agreed on, and it comes as no surprise that the chapter headings are, respectively; "Infection and immunity," "The immune reactions," "Antigens," "Serum proteins," "The production of antibody," "The antigen-antibody reaction," "Precipitation," "Agglutination," "Isohemagglutination," "Toxins and antitoxins," "Phagocytosis," "Cytolysis and complement fixation," "Antiviral immunity," "Allergy," and "Experiments in serology." This last chapter constitutes a brief laboratory manual.

Some omissions, doubtless deliberate, render the book less attractive for certain purposes. There is little account of the actual operation of immune mechanisms in disease; there is only fragmentary discussion of the use of serological methods in the diagnosis, treatment, and prevention of specific diseases; and the chapter on blood groups is rather incomplete. However, without these and other omissions the book could not have been kept down to its attractive size.

There are some debatable statements. On page 22, the discussion seems to ignore the abundant modern evidence that the "unitarian" theory is a great oversimplification; on page 46, old work on the role of lecithin and cephalin in precipitation, which has not been confirmed, is presented without comment; on page 84, the probably nonexistent nonspecific anamnestic reaction is presented as well established; and on page 179, it is stated that "most (Rh) typing serums in use today contain blocking antibodies, so saline must not be used as a diluent." The first part of this statement is true, but the second exactly reverses the consequences of this fact, which are that saline *must* be used as a diluent. Throughout the book, the author writes *haptene* instead of Landsteiner's actual (English) neologism *hapten*, which he meant to sound analogous to *antigen*. In this particular matter,

however, Carpenter sins in company with a number of our British colleagues. These and similar minor errors are not any more numerous than they are in the average textbook, and they do not seriously detract from the value of a well written and informative treatise, which the student should find very useful.

The book is well printed on good paper, with numerous clear illustrations. There is a good index.

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Methods in Enzymology. vols. I and II. *Preparation and Assay of Enzymes.* Sidney P. Colowick and Nathan O. Kaplan. Academic Press, New York, 1955. xxv + 835 pp and xx + 987 pp. Illus. vol I: $18; vol. II: $23.50.

With the tremendous growth of enzyme chemistry in recent years, interest in enzymes as indicators of cellular events or as analytic and clinical tools has spread to diverse scientific fields. However, these new applications have scarcely begun. Many investigators who might have utilized enzymes in some phase of their work have either not been aware of the potentialities or have been discouraged by the vast, widely-scattered literature of enzyme methodology. The compilation of this literature in *Methods in Enzymology* provides a working handbook for the preparation of enzymes.

The two volumes reviewed here are the first in a series of four that will cover the entire field of enzyme methodology, including many ancillary subjects such as chromatographic, physicochemical and other analytic techniques. The preparation and assay of enzymes is covered in volumes I and II, while the preparation and assay of substrates and special techniques for the enzymologist will provide the subject matter for the later volumes.

It is apparent that the ambitious aims of the editors give rise to numerous problems. The problem of presenting authoritative directions has been solved by a careful choice of authors. Thus, more than 200 contributors are represented in these two volumes. Each of these has worked in at least one phase of the field he reviews and often is the original author of much of the material presented. The problem of integrating the reports of these many authors so that duplication is avoided has been approached in two ways. The first is illustrated by the organization of the series. For example, substrates that are used for more than one enzyme system are considered separately in volume III. These are then omitted from the individual descriptions, and the appropriate reference to volume III (as yet unpublished) appears. A number of general preparative procedures are also given, and these are also omitted from reports on individual enzymes. Second, the various descriptions of individual enzymes are integrated by cross references in each article to appropriate parts of other articles. These two devices, plus an excellent author and subject index, constitute one of the most attractive features of these books. Without such organization, the subject matter could hardly have been covered in the space allotted. It should be noted that this integration means that all four volumes (or at least the first three) must be available to take advantage of these features.

Volume I contains four sections, as follows: "General preparative procedures," "Enzymes of carbohydrate metabolism," "Enzymes of lipid metabolism," and "Enzymes of citric acid cycle." The first section contains a wealth of material of general utility to the protein chemist. The review by R. K. Morton on methods of extraction of enzymes from animal tissues presents an excellent series of approaches to the first question one considers in investigations involving enzymes: How is the enzyme obtained in a soluble form? The various new techniques using butanol, which were devised by the author, are a significant, original contribution to this field. Other articles, on the preparation of various cell fractions, protein fractionation by solubility, and the use of adsorbents and resins in protein fractionation, outline the general procedures available for enzyme purification. Although some of this material has appeared previously in review articles, its inclusion here is needed in order to present a complete summary of the enzyme techniques that are available. The first section ends with a useful compilation of buffer mixtures by G. Gomori.

The order in which the various individual enzymes are described in the other sections follows, in general, metabolic sequences. For example, enzymes involved in polysaccharide metabolism are followed by descriptions of the monosaccharide kinases, then by mutases, aldolase-type enzymes, and so on. Each report follows a fairly standardized form, which includes, in order, assay method, purification procedure, and properties of the enzyme. Volume II continues the description of enzymes with five sections devoted to enzymes of protein metabolism, nucleic acid metabolism, phosphate metabolism, coenzyme and vitamin metabolism, and respiratory enzymes, respectively. This is a quite reasonable order of presentation of the material.

The list of enzyme preparations covered in each field is very complete and includes enzyme activities that can be assayed but that have not yet been separated to any degree. I have prepared two enzymes using these volumes as a text. In both cases, the directions were sufficiently complete so that the purification procedure given could be used successfully. Reference to the original literature was needed on occasion. The directions given here for the preparation of enzymes, in general, are more complete than those given in other handbooks of preparative methods. The detailed description of the assay procedures is particularly helpful. In a few cases, where a large number of enzymes from different sources are described (such as amino acid decarboxylases), the directions are somewhat less detailed and the original literature must be consulted. A spot check for errors in references revealed none. A few typographical errors were found.

One hopes that succeeding volumes of the series will match the high standard set by these two books. Volume III appears to be well integrated (based on the outline given) with the first two. The outline for volume IV does not appear as promising. Some of the subject matter falls outside the scope of the first three volumes and might well be omitted.

It is regrettable that the cost of this series will keep it off the bookshelves of most students, for the series contains so much that can be applied to daily research work. One could consider it a "dictionary" of methods that is dipped into almost daily, if only to find a reference.

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Advances in Virus Research. vol. III. Kenneth M. Smith and Max A. Lauffer, Eds. Academic Press, New York, 1955. ix + 338 pp. Illus. $8.

The policy of publishing intensive reviews of rather restricted topics has been continued in volume III of *Advances in Virus Research*. However, the paper by Seymour Cohen on "Comparative biochemistry and virology" is an exception, for it ranges broadly over the virus field, including plant, insect, and animal viruses as well as bacteriophages. The emphasis is on diversity rather than uniformity, and the author closes with the well-justified warning that reasoning by analogy is a poor substitute for observation. The paper by R. E. F. Mathews and J. D. Smith on the "Chemotherapy of viruses" is not only a good review of this subject but also a good review of the biochemistry of nucleic acids. A review of "Tumor viruses" by Beard, Sharp, and Eckert is more restricted than the title would indicate, being primarily a review of rabbit papillomatosis and chicken

erythromyeloblastosis. The brief article by K. M. Smith on "Morphology and development of insect viruses" is, as might be expected, stronger on morphology than on development.

The review by Maramorosch of the "Multiplication of plant viruses in insect vectors" is a convincing presentation of the evidence that certain viruses have both insect and plant hosts. Further evidence for this conclusion is presented in the paper by L. O. Kunkel on "Cross protection between strains of yellows-type viruses." Interference between related strains of the aster yellows virus occurs in the insect host as well as in the plant host. The chief criticism that one might make concerns the editorial policy of including a review on the "Current status of bacterial transformations" by Ephrussi-Taylor in a volume entitled *Advances in Virus Research*. There has been no dearth of good reviews on bacterial transformation in recent years, and there are many neglected topics that might more legitimately be classified as virus research. In science as in politics there is often a maldistribution of press agents.

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A Symposium on Inorganic Nitrogen Metabolism: Function of Metallo-Flavoproteins. Sponsored by the McCollum-Pratt Institute of the Johns Hopkins University. William D. McElroy and Bentley Glass, Eds. Johns Hopkins University Press, Baltimore, 1956. 728 pp. Illus. $10.

The sixth annual symposium of the McCollum-Pratt Institute is reported with the very high standard of excellence that is now expected. The subject is a very involved one with sufficient exact knowledge in some aspects to afford points of departure for the more unknown parts such as the reduction of nitrite to the level of ammonia.

Four leading topics are the reduction of nitrate and nitrite, the processes of denitrification, the nature of xanthine oxidase, and the significance of metalloproteins in electron transport. The functioning of molybdenum in nitrate reduction, xanthine oxidation, and nitrogen fixation is interwoven with the agricultural observations on its essentiality for plant and possibly animal nutrition. Other topics are nitrification, nitrogen fixation, and ammonia metabolism. There are 36 articles contributed by 62 authors, covering about all that is known on the several topics except that of electron transport. Each is well bulwarked with diagrams and tabular material.

Recorded discussions are an important part of these symposia. In this instance, they are extensive (60 pages) and serve to bring out interesting parts of the topics as well as to present some original short contributions. They are unusual, too, because they do not stray far from the central theme.

Bentley Glass again has supplied an excellent summary of the symposium (63 pages) showing his capacity both as an editor and as a dispassionate student of what others find. If one has an interest in the subject but is not a research worker in any of the several aspects, he should read this summary first and thus be led back to the original works.

An instance singled out by Glass exemplifies the tone of the work. Speaking of the work of Helmut Beinert and Frederick L. Crane on fatty acid oxidation, he quotes, " 'When we proceed to "isolate" these enzymes we wreck this whole edifice and obtain the associated flavoprotein dehydrogenases as one fragment and the connecting leads, electron transferring flavoprotein and the diaphorase, as another fragment. The terminal electron-transferring system is what we flush down the drain as insoluble. . . . We are then surprised about the headaches we experience in trying to reassemble the parts.' " Glass adds on his own account, "Perhaps the most significant change in point of view in biochemistry in recent years is, in fact, the growing realization on the part of biochemists that they will ultimately have to take into account the existence of organization on the biological level."

An adornment of the time is surely the great progress that is being made in all aspects of enzymology and in knowledge of the pathways of biological reactions. No more faithful picure can be gained of this scene than from the reports of these symposia.

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The Blood-Brain Barrier with Special Regard to the Use of Radioactive Isotopes. Louis Bakay. Thomas, Springfield, Ill., 1956. 154 pp. Illus. $5.50.

This book is primarily concerned with the penetration of radioactive ions into normal and abnormal brain tissues. The book is written within the framework of the author's personal experience with the uptake of phosphorus-32 labeled inorganic phosphate; only two pages are devoted to the uptake of other ions by brain tissue. Most of the information presented was derived by the author from autoradiography of brain slices and from direct counting of brain tissue samples following the intravenous injection of radioactive phosphate. Of the 32 illustrations, 18 are autoradiographs and 12 depict the temporal and spatial distribution of phosphorus-32 following its injection into patients and experimental animals. The author has had considerable experience with the differential uptake of phosphorus-32 by brain tumors, and the section of the book that deals with this topic will be of particular interest to neurosurgeons.

The book is written loosely from the point of view of style, organization, and scientific accuracy. Permeability is treated throughout in terms of penetration rates without consideration of either the driving force or the area available for the penetration process. In addition to conceptual errors, there are numerous grammatical mistakes and typographical errors. Direct quotations from other authors are not always provided with quotation marks. The comprehensive title and chapter headings in the table of contents may be somewhat misleading. There are 12 chapters that nominally cover the history, anatomy, physiology, and pathology of the blood-brain barrier. Actually, the book deals only superficially with these topics, and the reader who seeks a scholarly presentation of the field as a whole may be better advised to read the excellent chapter on cerebrospinal fluid given in Davson's *Textbook of General Physiology*.

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Bone, an Introduction to the Physiology of Skeletal Tissue. Franklin C. McLean and Marshall R. Urist. University of Chicago Press, Chicago, Ill., 1955. xii + 182 pp. Illus. $6.

This is the first of a series of publications in *The Scientist's Library: Biology and Medicine*. The object of the series can be inferred from the following statement by Peter P. H. De Bruyn in the preface: "The authors have been asked to emphasize introductory concepts and problems, and the present status of their subjects, and to clarify terminology and methods of approach instead of limiting themselves to detailed accounts of current factual knowledge. The authors have also been asked to assume a common level of scientific competence rather than to attempt popularization of the subject matter."

The topics covered in this book are bone as a tissue; histogenesis of bone; structure and chemical composition of the bone matrix; chemistry and crystal structure of the bone mineral; dynamics of calcification; enzymes and bone; re-

sorption of bone; hormones and bone; mineral metabolism; radiation, isotopes, and bone; postfetal osteogenesis; the healing of fractures; and pathologic physiology of bone. A bibliography and an index are appended.

Biology and Medicine

Physical Techniques in Biological Research. vol. I. *Optical Techniques.* Gerald Oster and Arthur W. Pollister, Eds. Academic Press, New York, 1955. xiii + 564 pp. Illus. $13.50.

This book is the first volume of a comprehensive series addressed to research workers in the biological sciences whose over-all objective is to present a description of the more recent developments in physical methods of analysis and instrumentation. In the words of the editors, "It is hoped that this treatise will serve as a real orientation for biologists and for chemists and physicists who may be potential biologists."

Following the modern trend of treatise preparation, each chapter is written by an individual contributor who is an expert in his particular field. In view of the complexities and diversity of modern instrumentation, the multi-author approach is to a large extent inescapable, and certainly one which facilitates the completion of the treatise shortly after its conception. Alexander Pope's lines: "One science only will one genius fit; so vast is art, so narrow human wit," written in an age when experimental science was in its infancy, becomes more difficult to refute with the passage of time and the accumulation of classified knowledge. This frailty of the human mind is apparently limited to the compilers of encyclopedias. It is a limitation to which book reviewers as a genus have not subscribed, and I have no intention of establishing a dissenting precedent.

The first chapter of the present volume consists of a comprehensive review by Jerome L. Rosenberg of photochemistry and luminescence, covering both the mechanism of photochemical reactions and the associated experimental techniques that are employed in their study. The bulk of the volume is composed of a series of related essays dealing with light scattering, birefringence and dichroism, absorption spectroscopy in the visual region, followed by independent treatments of ultraviolet and infrared spectrophotometry. The latter section, by Carl Clark, is particularly comprehensive; it also contains a brief description of infrared microscopy with Burch-type reflecting optics as applied to the absorption spectra of large cells. The several chapters contain a wealth of information that at times is critically evaluated by the authors; all chapters contain individual bibliographies of the fields surveyed.

The volume also covers the theory and operation of the light microscope, (phase and interference microscopy) and terminates with a chapter on electron microscopy. These sections are especially well prepared, present the reader with a mathematical background of the optical principles for the diverse types of lens systems, and also describe the practical adjustments for securing optimum performance. The section on the light microscope, by L. C. Martin, discusses the working distance of objectives and means for its increase by the Dyson reflecting attachment. However, no mention is made of the availability of "nuclear-type" oil-immersion objectives with working distances up to 3 millimeters and possessing an N.A. of 1.0. These refracting objectives, which are designed primarily for the study of tracks recorded by nuclear particles in very thick photographic emulsions, are equally effective in the examination of tissue autoradiographs and in *in vivo* histological techniques that necessitate a large separation between the front element of the objective and the lower layer of the sample.

It is difficult to evaluate the over-all effectiveness of this treatise, on the basis of the contents of the first volume, as a means of bridging the hiatus between the physicists who created the modern working tools and the investigators in the biological sciences whose work would profit by their application. The contributors have expended considerable effort on a lucid presentation that is further benefited by an unusually large number of charts and line drawings. Nevertheless, the book is not an easy one to read. It is a work that must be studied and one that is well worth the reader's effort.

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Embryologie. Ein Lehrbuch auf allgemein biologischer Grundlage. Dietrich Starck. Georg Thieme, Stuttgart, 1955. xix + 688 pp. Illus. $18.55.

Several decades ago, most textbooks of embryology were essentially descriptions of the serial changes in the morphological mold that an egg undergoes on the way to becoming a free-living individual. Textbook embryology was natural history of the initial phase of the life span, presented, like old-fashioned history in general, as a chronological record of events, with no more than side glances given to the forces, mechanisms, and interactions that do the molding. Meanwhile, the active study of development has become ever more analytic, with the center of interest shifting steadily from chronological seriation to causal explanation. Not just what happens, but precisely how and why it happens as it does, has become the focal issue. Moreover, developmental principles and mechanisms were found to be in operation way beyond the embryonic period, as, indeed, they form the rational basis for the understanding of pathological variations and subsequent repair throughout the life span. There has thus gradually come about a great widening of scope and outlook in the field of embryology. For such a reorientation at the research front to permeate the teaching process, and eventually the textbooks, quite commonly takes decades. The present text proves very convincingly that, for the teaching of embryology, this lag period has at last passed.

The book is the product of a scholarly effort to combine the disciplined description of detailed facts with a modern treatment of their underlying dynamics, and the effort has been crowned with admirable success.

Here is a book that conveys both information and understanding, treats its subject with breadth as well as depth, and above all, with a fine sense of proportion. In particular, it has resisted the temptation to make concessions to the modern analytic approach at the expense of rigorous treatment of morphological facts. There is always the danger that preoccupation with causal explanations may dim the exposition of the concrete factual phenomena that are to be explained. Some recent publications point up this danger. The present text, however, has carefully avoided it. It has struck a satisfactory balance between descriptive detail and excursions into the areas of genetics, cytology, physiology, endocrinology, experimental morphology, and evolution, through which the observational data are interwoven into an intelligible fabric. In full recognition of the fragmentary state of this fabric, the author has placed the emphasis on problems rather than on solutions, and he has been careful to present controversial issues as unresolved, without bias in favor of future solutions not warranted on the basis of existing evidence.

Quite naturally, such a broad treatment of the field, with equal attention to all three of its parameters—namely, morphological description, causal analysis, and evolutionary interpretation—required restraint in the choice of pertinent examples. Again, the choice has been very fortunate. There would be no point, therefore, in complaining that, for instance, histogenesis has been given only cursory consideration, for it could not possibly have been included without seriously weakening the rest of the offering. There are certainly no major omis-

sions within the chosen limitations. The examples have been chosen from such a wide array of biological areas that they will leave the student with the firm realization that embryology is an integral part of the indissoluble continuum of the biological sciences. The following casual list of items may illustrate how far this text has ranged into the domain of general biology.

Ovarial eggs of mealworms, to illustrate yolk relations; detailed chromosome chart of *Drosophila*; a fine discussion of biochemical genetics, stressing the contributions of both the Beadle-Tatum and the Kühn-Butenandt teams; morphogenetic contributions of nucleus and cytoplasm as studied in the alga *Acetabularia*; sex determination in moths and worms; embryonic determination in the sea urchin, expanded to five pages; Holtfreter's analysis of blastomere movements, likewise given five pages; studies on the causation of deformities and monsters, including the effects of the maternal milieu; a very competent account of comparative embryogenesis and placentation in different forms; endocrine regulations of estrus, not only regarding the underlying physiological mechanisms but in the context of the totality of biological and ecological conditions through which rates of development, nutrient perquisites, state of completion at birth, and other embryogenetic and endocrine events have gained their adaptive significance; pigment patterns in amphibians and birds; and Wolffian lens regeneration in amphibians.

Even such very recent developments as the experimental production of a whole rabbit from a single blastomere of the two-cell stage by Seidel, the demonstration of the derivation of ventral body musculature from the lateral plate in birds by Rawles and Strauss, and the evidence of functional polarization of the retina shown by Sperry and Stone, have been included.

The organization of the material follows customary didactic lines, as may be seen from the following topical outline (giving approximate proportions in percentage of total space):

1) General part. (i) "Germ cells and germ cell formation"; "Fertilization, genes, sex determination" (13 percent). (ii) "Cleavage" and (iii) "Gastrulation and embryogenesis in holoblastic forms," including germ layers, "organizer" actions, teratology (11 percent). (iv) "Early development of meroblastic forms," and (v) "Origin of blood, vessels, and mesenchym" (7 percent). (vi) "Early development of mammals," including comparative treatment of placentation (21 percent). (vii) "Ontogeny and phylogeny" and (viii) "Evolutionary evaluation" (3 percent).

2) Special part. (i) "Nervous system and receptors," including integument (16 percent). (ii) "Alimentary tract and respiratory organs," including coelom (9 percent). (iii) "Urogenital system" (5 percent). (iv) "Circulatory system" (7 percent). (v) "Skeleton and musculature" (8 percent).

The illustrations are clear and informative and in large part are originals. The language is concise and straightforward. Some tables of interesting systematic and comparative data are appended. There are 45 pages of literature references, including approximately 2000 citations, pertinently selected and carefully edited.

The author states that "the book addresses itself not only to the student, but to all those in medicine and biology who want an introduction into the problems of development, not just a compilation of data." It can be said that he has satisfactorily met his goal. He has produced what may rightly be considered a companion volume to the *Textbook of Anatomy* by Braus, published a third of a century ago, bridging the classical and the modern ages by doing justice to both and adopting the errors, biases and extremes of neither.

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Annual Review of Entomology. vol. I. Edward A. Steinhaus and Ray F. Smith, Eds. Annual Reviews, Stanford, Calif., 1956. 466 pp. $7.

This book, in all probability, is one of the most important and certainly one of the most useful general reference works on entomology to appear in a long time. The membership of our association and our colleagues everywhere will welcome its appearance with pleasure. Those of us who have had to struggle with the literature of entomology have long realized the need for a work of exactly this type. It has been conservatively estimated that there is appearing in world scientific literature well over 4000 papers on entomology each year. There is reason to believe that the actual number is considerably greater than this and that it will continue to grow. Some of this literature is scattered and inaccessible, and much of it appears in journals that are not primarily concerned with insects. Clearly, the mass of literature accumulating in all the various fields of entomology is beyond the ability of any one person to obtain readily, to read thoroughly, and to appraise critically. There has been, in recent years, a growing realization of this fact among entomologists generally.

It is regretted that space limitations here forbid even brief discussion of any of the 21 papers making up this volume. However, some idea of the scope, the subjects, and the selection of authorships may be gained by enumeration of the papers given in the newly issued volume I of the series: "The physiology and biochemistry of diapause," by A. D. Lee, University of Cambridge, England; "Insect nutrition," by H. Lipke and G. Fraenkel, University of Illinois; "The language and orientation of the honeybee," by K. von Frish and M. Lindauer, Universität München, Germany; "The stability of scientific names," by R. L. Usinger, University of California; "Some aspects of geographic variation in insects," by T. H. Hubbell, University of Michigan; "Arthropod resistance to chemicals," by W. M. Hoskins and H. T. Gordon, University of California; "The mode of action of insecticides," by C. W. Kearns, University of Illinois; "The chemistry of insecticides," by H. Martin, Canada Department of Agriculture; "Persisting insecticide residues in plant materials," by F. A. Gunther and R. C. Blinn, University of California; "Repellents," V. G. Dethier, Johns Hopkins University; "Soil insects and their control," by J. H. Lilly, Iowa State College; "Stored products entomology (The assessment and reduction of losses caused by insects to stored foodstuffs)" by E. A. Parkin, Pest Infestation Laboratory, Slough, Bucks, England; "Apparatus for application of insecticides," by J. L. Brann, Jr., Cornell University; "Ecology of forest insects," by S. A. Graham, University of Michigan; "Some recent advances in apicultural research," by C. G. Butler, Rothamsted Experiment Station, England; "Insect transmission of plant viruses," by F. F. Smith and P. Brierly, U.S. Department of Agriculture; "Nonbiting flies and disease," by D. R. Lindsay, National Institutes of Health, and H. L. Scudder, U.S. Public Health Service; "Veterinary and medical acarology," by H. S. Fuller, Walter Reed Army Medical Center; "Modern quarantine problems," by A. F. Camp, Florida Citrus Experiment Station; "The fundamental theory of natural and biological control," by W. R. Thompson, Commonwealth Institute of Biological Control, Canada; and "Effect of pesticides on balance of arthropod populations," by W. E. Ripper, Fisons Pest Control, Ltd., Cambridge, England.

Each paper has been accompanied by appropriate bibliography, making a total of 2308 references. These vary from 11 to 269, but average approximately 110 references per paper. Author and subject indexes to the volume also are included. As would be expected in the first volume of a work of this kind, there may be found here and there occasional lack of uniformity not only in the selection but also in arrangement of material—this occurs

both in the text and in the bibliographies. Then too, in this first volume, the word "Annual" in the title should not be taken very seriously, for it was necessary in numerous instances, in order to make the subject matter complete, that a much longer period of time should be included —in some cases, the author had to go back several years to make the narration fully understandable. Obviously, this will not be needful in later volumes of the series, once the subject has been discussed and brought down to date in a previous volume within the series.

Also, despite some fears to the contrary, it seems obvious that this newly inaugurated Annual Review series ought not to have adverse effects on any other publications in related fields—it covers a type of reference work that is not duplicated elsewhere. Now that a start has been made—now that the ice has been broken—it is easy to visualize that each succeeding volume of this series will average up gradually better in every way than those previously published, that minor flaws will be eliminated, that the scope of the whole plan will be expanded, and that the series will gain steadily in practical utility and general usefulness.

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Traité de zoologie. Anatomie, systématique, biologie. vol. XVII. *Mammifères.* Les ordres: anatomie, éthologie, systématique. Parts 1 and 2. Masson, Paris, 1955. 2300 pp. Illus. Paper, F.22,000; cloth, F.23,600.

The seventeenth volume of this important series of zoological monographs is devoted to a systematic account of the living and extinct orders of mammals. It is a large and impressive contribution. Some idea of its magnitude may be gained from the fact that the two bulky fascicles include a total of 2300 pages and are illustrated with 2106 text figures and four colored plates.

In scope and manner of presentation, there is a strong resemblance to the second volume of Max Weber's *Die Säugetiere*, which in its editions of 1904 and 1928 has served as a primary source book for two generations of mammalogists. This volume of the *Traité de zoologie*, along with the forthcoming sixteenth volume that is to be devoted to anatomy and reproduction, promises to provide a worthy successor to Weber's monumental work. The thirteen authors who have collaborated to produce it are leaders among modern French zoologists and paleontologists.

The orders of mammals are treated in sequence and in remarkable detail, which extends in some instances down to the generic and even the specific level. The number of pages allotted to each order reflects in general the amount of zoological (and to a lesser extent paleontological) knowledge that is available for it. For example, the presentation of the order Insectivora totals 139 pages and consists of sections on anatomy and reproduction (68 pages, 89 figures), zoological affinities (11 pages, 1 figure), systematic accounts including habits and distribution (45 pages, 34 figures), fossil history (7 pages, 11 figures), and bibliography (8 pages, 238 titles). Relatively obscure fossil orders such as Astrapotheria, Embrithopoda, and Tillodontia are each disposed of in two or three pages. The section on primates including man occupies 353 pages and in itself is a major contribution to the fields of primatology and physical anthropology.

The system of classification adopted here represents an adroit blending of old concepts of classical zoology and the revolutionary modern ideas of paleontology. Some decades ago the initiative in mammalian classification at the ordinal and familial levels passed from the hands of zoologists into those of paleontologists, whose investigations reveal actual rather than speculative lines of evolution. Adjustment to radically new arrangements of orders has been especially difficult for those zoologists who are encumbered by pedagogic tradition. The centuries-old concept that a system of mammalian classification must culminate with the Hominidae would find few champions among modern taxonomists, yet it is honored in the present treatise at the expense of a necessary reallocation of orders related to the primates. Controversial groups such as the monotremes and the tree shrews are comfortably left in their traditional niches. Some suprageneric groups have received new names.

Anatomy and reproduction, although primarily reserved for another volume in the series, are treated at some length and in a highly competent manner in each account of a recent order. Habits, habitats, and geographic distribution, which can be most satisfactorily dealt with at the specific level, receive rather cursory treatment. There is no general coverage of subjects such as zoogeography and population dynamics, which claim a large share of the interest of modern mammalogists.

It is inevitable, in a compilation so large and so complex, that errors should occur. One may note, for example, the statement on page 7 that the geographic range of perameloid marsupials includes New Zealand, and on page 1653 that Simpson's classification of 1945 is followed in placing the Tupaioidea in the Insectivora. Fortunately these slips are not repeated in the detailed accounts of the respective groups.

The text figures, including drawings and halftone photographs, are remarkably clear and greatly enhance the usefulness of the work. The bibliographies are extensive and well chosen; they include some surprisingly recent titles.

Teachers, advanced students, and systematic mammalogists will find this treatise to be an indispensable reference and source book. Its publication represents an important milestone in mammalogy.

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Evolution, Genetics, and Man. Theodosius Dobzhansky. Wiley, New York; Chapman and Hall, London, 1955. ix + 398 pp. Illus. $5.50.

This is an excellent text book for college students. It develops the orthodox genetic point of view on evolutionary mechanisms but includes enough of the dissenting opinions to show why this field of research is now very active and interesting. As befitting a textbook, a list of references to more comprehensive treatments of the various subjects that will be of interest to the reader is included at the end of each chapter. But this short book is more than a text. Dobzhansky has written a scholarly book that is fun for any educated man to read.

In view of the fact that the theory of evolution is the great central indispensable theory of biology, it is necessary and important to explain it to all men. Dobzhansky's wide interests, always centering around evolution, range from human creative effort such as the frontispiece reproducing Michelangelo's "Creation of Adam" and Old Stone Age art on the walls of the Lescaux caves to the short concluding discussion of the relation of science to ethics. The following quotation from the preface clearly states the intent and central theme of the book: "Biological evolution is a part of the evolution of the cosmos. The rise and the development of mankind are a part of the story of biological evolution. Man cannot reach a valid understanding of his own nature without a knowledge of his own biological background. It may, then, be that the study of evolutionary biology is the most important practical endeavor open to the human mind. Accordingly, an effort is being made in this book to show to the student that biology is not only a craft which is interesting to technicians and devotees but also a part of the fabric of modern humantistic thought."

Until this book appeared, there was no simple textbook of evolution that was centered around the genetic mechanisms. Dobzhansky has carefully integrated

many of the contributions of paleontology, anthropology, ecology, and the history of evolutionary thought with recent developments in population dynamics and adaptation.

There are a number of well-chosen illustrations that add to the interest of the text. In such a large framework, some subjects and much pertinent information is necessarily omitted in a short text. Nevertheless, the material covers most of the genetic and much other information that is very pertinent to an appreciation of current theories of evolution. The reader who is struck by Dobzhansky's optimism in his discussion of what is called "evolutionary ethics" may enjoy comparing his views with those expressed recently by Darlington in his volume *Facts of Life*. Evolution has produced mankind; in studying his greatness and limitations, we need to respect and understand evolution. Dobzhansky's *Evolution, Genetics, and Man* will help many students to do so.

WILSON S. STONE
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The Recent Genera of the Caridean and Stenopodidean Shrimps (Class Crustacea, Order Decapoda, Supersection Natantia) with Keys for Their Determination. (No. 26, Zoologische Verhandelingen) L. B. Holthuis. E. J. Brill, Leiden, Netherlands, 1955. 157 pp. $3.60.

Students of the decapod Crustacea throughout the world will enthusiastically welcome this review of the recent genera of the caridean and stenopedian shrimps of the world. Its keys bring up to date the corresponding portion of Borradaile's "Classification of the decapod Crustacea" [*Ann. and Mag. Nat. Hist.* **19**, 457 (1907)] which, however, did not include categories below subfamilies; it goes beyond that classic endeavor in diagnostically keying out all known valid genera. For each genus, the type is indicated, and a pertinent though not exhaustive synonymy is given, which includes all changes in the spelling of the individual generic names. Highly commendable are the results of the effort that was made to illustrate each genus with a typical, where possible the type, species, usually by reproducing the original or best published figure or figures; the publications in which they appeared are conveniently listed by authors; in two instances, original drawings were prepared by the author for this paper. I find nothing to criticize in this exceptionally well done piece of work.

In itself a masterly contribution, it may be thought by some to be a relatively small one in view of the immense number of genera in zoology; however, in the aggregate, it is an exceedingly significant basic step, one might say, toward the eventual realization of the much-needed synoptic treatment of the plant and animal kingdom that has been so earnestly advocated by T. K. Just of the Chicago Museum of Natural History before the National Science Foundation and elsewhere.

It is to be sincerely hoped that this is but the forerunner of a series of papers that, in time, will encompass, at least, the remaining genera of the Crustacea Decapoda.

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Medical Research: a Midcentury Survey. vol. I. *American Medical Research: In Principle and Practice.* xxxii + 765 pp. vol. II. *Unsolved Clinical Problems: In Biological Perspective.* xxxii + 740 pp. American Foundation, New York, 1955. $15.

When a book fits no standard pattern, it is perhaps the reviewer's first duty to say what sort of book it is. The answer would be that this is a book of essays discussing medical research from every angle: scope, where done, support, objectives, and problems. They are good essays, not to be read at a sitting but to be savored of an evening. They are the product of 15 years of thought and compilation by Esther Everett Lape and her staff in the American Foundation, aided by consultants who included several Nobel laureates.

The thesis of the volumes is that medical progress comes through research and that, in the main, fundamental research at basic levels precedes clinical applications.

The first volume starts with and stresses medical research as related to biological, chemical, physical, and mathematical science. It goes on to a series of philosophical discussions of the meaning of research and to means of financing research, with particular reference to the influence of government. Then comes the principal section on the agencies that conduct research: universities, their medical schools, foundations, and institutes. This is really fascinating reading because of the illustrative detail from many institutions. Finally, there is a section on official and unofficial standardizing agencies and patent policies. One can pick nearly any subject in medical research, or in fact in medical education, and find it discussed in different settings through the various chapters. I cite the following examples of discussions on the problem of medical faculties and their teaching and service loads:

"The principle of full-time men as heads of preclinical departments is everywhere accepted. In clinical departments, a major obstacle to full time has been the great disparity between a full-time salary and the amount a part-time clinical teacher can earn from private practice—according to some reports, from five to ten times the amount received for teaching. Yet of the value of full time in clinical as well as in preclinical departments there has been significant evidence. In one academic council debating whether or not to continue full time, a dean testified that by virtue of the full-time system the clinical departments of that school had become, in the broadest sense, university departments, carrying on teaching and research together, in accordance with graduate school standards" (p. 123). "Among dangers . . . in the practice of medicine by faculty members as a source of income for the medical schools is the possibility that the faculty may be selected on the basis of earning rather than of teaching capacity" (p. 153). ". . . all such arrangements simply put the medical school in the position of operating a business and sharing in the income; or of making the men who earn the income contribute to their own salaries or to the support of their departments. . . . All service beyond that necessary for teaching and research [is] alien to the primary function of education" (p. 272).

On the other hand, Phemister (University of Chicago) was quoted, ". . . under existing economic and social conditions in the United States, the most promising way of gradually placing education in clinical medicine on a uniform basis of organization and on an educational level that most nearly approximates the educational level of other university departments appears to be by the employment of full-time group practice for the clinical department of the medical school" (p. 272).

On university relationships, the following may be cited: "However remarkable the developments for enriching and enlarging observation outside the hospital, medical education designed to produce practicing physicians is not likely to dispense with the hospital . . . [but] there are those that would stress the need of drawing the medical school closer to the university for the stimulus and sustenance medical progress derives from biology, genetics, animal husbandry, chemistry, physics, psychology, anthropology. Medical schools today are, on the whole, better described as attached to rather than integrated with universities. The growing volume and significance of the contribution to medical research from nonmedical university divisions and the advantages in the scientific training procurable in many of these divisions have prompted some to ask whether the urgent present need may not be a bold experi-

ment in integrating medical schools with universities on the theory that segregated medical schools organized primarily to produce practitioners may not produce the best medical training" (p. 275).

The second volume turns from the general to the specific. Nine major baffling subjects are reviewed in detail and with a surprising virtuosity: background, present state, and anticipations. Investigators are in general curious about the status in fields other than their own, but often find reviews in other fields written by investigators for investigators somewhat incomprehensible. The discussions in this volume will be helpful and orienting to them and also to many others who are interested in health problems, in foundation work, and in the work of government agencies.

The nine problems are covered in sections of perhaps 50 pages each, which are in the main composed as annotated compilations of the literature rather than as highly original interpretations.

The first section, cancer, slights neither fundamental problems in growth, nor, at the other extreme, chemotherapy, and it places the emphasis of the latter properly at preponderantly a preclinical level of development. The next section, on infertility, hardly discusses the problem of fertility, surely a basic problem in the world today. The problem of how to adjust populations to resources by better means than war, plague, and even improved production and distribution presses for an answer. Arteriosclerosis, hypertension, and the rheumatic syndromes, the next three sections, skirt the problem of aging, another fundamental.

The two sections on tuberculosis and virus diseases exemplify the present state of infectious diseases: tuberculosis, the bacterial representative, still a problem but well on the way to solution; and the smaller virus diseases, which are as yet beyond chemotherapy. No one knows the fundamental difficulty in treating virus infections. It is probably not simply the intracellular position of the invader; more likely it lies in the fact that the virus particle possesses life only when it can borrow the enzyme systems of the host cell, and thus is inseparable from the cell. Perhaps one must sacrifice the cell if one is to kill the virus during its intracellular period.

The last two sections, alcoholism and schizophrenia, are intensely interesting. Next to the problem of fertility and population, mental troubles head the list of human disabilities. We are all conscious of the present hints of biochemical as well as the more traditional psychopathological mechanisms in the mind, and these are well touched upon:

"The present intensified interest in the effects of drugs in schizophrenia or other forms of mental illness is due, in part, to immediate therapeutic hope, and, in greater part, to the promise which some of these drugs seem to offer of shedding light on the biochemical mechanisms involved in mental disorder" (p. 650).

After a reviewer states what is in a book, he should say how good it is, what its uses may be, and what are its faults. The subjects are excellently presented, truly a major accomplishment. The book's usefulness is obvious, both as a source of thoughtful pleasure and of fact. The faults I do not find serious enough to mention.

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Shock and Circulatory Homeostasis. Transactions of the fourth conference 6–8 Dec. 1954, Princeton, N.J. Harold D. Green, Ed. Josiah Macy, Jr., Foundation, New York, 1955. 291 pp. Illus. $5.

The chapters included in this report are "Action of epinephrine in man," by Henry Barcroft; "The circulation in the periphery," by Hugh Montgomery; "Mesenteric lymphatic dynamics in the rat," by Silvio Baez; "The circulation in the splanchnic area," by J. D. Myers; "The pulmonary circulation," by André Cournand; "The pulmonary circulation in hemorrhagic shock," by J. E. Merriman; and "The aortic and coronary blood flow," by Donald E. Gregg.

Social Sciences

Research Frontiers in Politics and Government. Brookings lectures, 1955. Stephen K. Bailey, Herbert A. Simon, Robert A. Dahl, Richard C. Snyder, Alfred de Grazia, Malcolm Moos, Paul T. David, and Donald B. Truman. Brookings Institution, Washington, 1955 vii + 240 pp. $2.75.

This small volume surveys the new research techniques and theoretical developments important to the extension of knowledge of politics and government. The specialist will find this a convenient summary, and the layman will get an insight into the vast research area bearing directly and indirectly on politics, government, and political behavior. The professional will find some gaps, the layman some confusion, and both enough interest to warrant the reading of the 1955 Brookings series.

Like most frontiers, those surveyed in *Research Frontiers* are often not well-marked. As a result, the sound of border warfare comes from its pages. While all eight contributors are political scientists, much of the research into political behavior bears little resemblance to the traditions of the field. Indeed, I felt some concern lest my colleagues in political science find themselves occupationally displaced by the psychologists, social anthropologists, and sociologists. But the final contributor, David B. Truman, effectively restored the balance.

Stephen K. Bailey of Princeton opens the series by surveying familiar ground—the relationships between academicians and operators and the approaches to research in politics and government. In his contribution, "Recent advances in organization theory," Herbert A. Simon of the Carnegie Institute distinguishes between programed and nonprogramed decision-making. The object is to devise organizational structures to get the right decisions. Experiments along this line are surveyed, all of which might suggest to the layman both the danger of over-organization and the advantage of inefficiency that might make it tolerable.

Decision-making, the meaning, and the distribution of power are considered on a far grander scale by Robert A. Dahl of Yale in his "Hierarchy, democracy, and bargaining in politics and economics." These control systems, together with the price system, are viewed as complementary and not as exclusive power arrangements or social techniques. Reality is mixed; all techniques of control are used. This is true enough, but stressing this obvious point too much has its dangers. The pure models excluded complementarity, but stressing the mixture tends to obscure the vast differences in control systems.

New frontiers are surveyed by Richard C. Snyder of Northwestern University in his "Game theory and the analysis of political behavior." The practical politician and the professional bureaucrat "play the game" instinctively. Game theory promises to sharpen instinct and even to correct it. Now even the scholar might play the great game of politics vicariously. Snyder is modest in his claims. Game theory has yet to prove its utility. He suggests some interesting applications in international politics where the theory might throw some light on situations that are shrouded by security regulations. Moreover, in conflict situations with varying strategies available to both sides, with a range of possible outcomes depending on the pairing of strategies, and a range of pay-off values, policy makers might become more explicit and less vulnerable to surprise by the application of game theory.

Alfred de Grazia of Stanford, in "Research on voters and elections," surveys the techniques of probing voter behavior. This interesting contribution closes with the disquieting observation that "Traditional democracy is being slowly crushed in the gigantic pincers of depolitization and totalitarianism." But this feeling is partly offset by Malcolm Moos of Johns Hopkins who, in "New light on the nominating process," gives a reasoned

defense of the convention system as an expression of democratic forces. Paul T. David of Brookings surveys recent research at the state level in his "Comparative state politics and the problem of party realignment." David B. Truman of Columbia presents an interesting concluding paper, "The impact on political science of the revolution in the behavioral sciences," that rescues political science from the psychologists, social anthropologists, and sociologists.

This small volume covers much ground. A short review can do scarcely more than point to the chapter headings. The papers give much emphasis to voter behavior and administrative practices. But little attention seems to be given to the political forces behind the voter and the bureaucrat. Nor is attention given to the absorbing problem of the bureaucrat and the political administration. Who really runs the show? The relationships between the Congress and the bureaucracy afford another avenue of fruitful research that is not mentioned here. Some things must be dropped in any short series of lectures, and this one did not drop as many as might be expected.

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A Solomon Island Society. Kinship and leadership among the Siuai of Bougainville. Douglas L. Oliver. Harvard University Press, Cambridge, Mass., 1955. xxii + 535 pp. Illus. + plates. $10.

The Siuai are Melanesian people who live in southwestern Bougainville in the Solomons. They constitute a discrete and self-conscious social aggregate sharing common linguistic and general cultural features, but they lack any formal organization; that is, there is no Siuai political group. In 1938–39 there were 4658 Siuai, and for 18 months during that period Douglas Oliver and Eleanor Schirmer Oliver lived among them and collected the data, some of which were presented as "Studies in the culture of Bougainville" [*Papers of the Peabody Museum of Archeology and Ethnology* **29** (1949)], but most of which have been cast into the present volume, a superb ethnography.

Oliver's objectives in writing *A Solomon Island Society* were threefold: to summarize the whole culture of Siuai; to fill a pedagogic need for reliable ethnographic descriptions of whole cultures; and to present much-needed substantive material on the subject of political institutions in stateless society. I am not expert in Oceanian culture, but I believe that Oliver has brilliantly achieved his goals. Furthermore, the modesty and honesty of Oliver's presentation and the demonstration in other publications of the author's expertness in things Oceanic lead to the conclusion that the previous judgment will be confirmed by specialist colleagues.

The present study of Siuai describes the society that existed in southwestern Bougainville at the time of investigation. The author, lacking firsthand observation of this culture since the war ("What if anything remains of the Siuai culture . . . I do not know") wisely refrains from comment on the more recent period. He does, however, set Siuai into the framework of expanding European influence in Melanesia. It is, he tells us, in the second stage of Westernization. The first stage, which lasted until 1915, saw ephemeral contacts with white traders; the second, much more significant, was dominated by the imposition and maintenance of peace by the Australian government (a cosmic irony in view of the events of 1942–43 in the Solomons). This *pax Australianus* is regarded by Oliver as one of the most potent forces that subverted the older Siuai polity and led to the emergence of the system he portrays.

Siuai subsistence is based on taro, which furnishes 80 percent of the diet and which demands more Siuai labor than any other occupation. The Siuai cultivate a field once, then allow it to lie fallow for a minimum of 5 years. There are several alternative designs to the pattern of shifting fields, but even under the most ideal system known to the Siuai at least five units of land not under immediate cultivation must exist for every unit in operation, or there is danger of famine. Oliver makes it clear that population in this area is far from the saturation point in terms of the productive capacity of native agriculture, but food shortages can occur locally. If the reader is puzzled at the seeming contradiction, he is urged to read Oliver's brief account of the labor involved in clearing virgin ground—it will then become quite clear why the Siuai men prefer to return to previously used land.

As a matter of fact, the field-clearing job done by the Siuai men was much more onerous and time consuming before their contact with Europeans. The difference can be laid to the greater efficiency of the new metal tools. This change, in combination with the effects of the *pax Australianus*, is said by Oliver to be responsible for the major changes that he believes have occurred within Siuai society in the past several decades. But before Oliver permits himself such generalizations and conclusions, he has taken us through a detailed and very neatly organized treatment of the major aspects of Siuai culture.

The book begins with a general orientation—a concise treatment of the ecological background, followed by an effective picture of the world as it is seen, more or less, by a Siuai. This is managed by a clever manipulation of native mythology and genealogy against a background of analytic insights provided by the anthropologist.

The author then turns to the social organization, bringing the material to the reader through ever-widening frames of reference, households, hamlets, larger kinship groups, and, finally, associations. Next, he returns to Siuai economy and lays the basis for an extensive treatment of leadership, its structure and functions. The book concludes with general statements and conclusions of several different types, only one of which is represented by the foregoing remarks.

Among Oliver's most interesting generalizations is his thematic analysis of Siuai, which, despite merely tangential reference to Morris Opler, is strongly reminiscent of that author's treatment of Chiricahua Apache. There is an important difference, however, in Oliver's division of Siuai ideology into four distinctive and fairly discrete sets of themes dealing with kinship, ranking, Australian-derived administration, and "mission ideology." These different sets of themes may conflict, and, indeed, Oliver indicates that acculturation may be in part described in terms of the shifting significance of the various sets of themes. He does not, however, see these ideologies as motive forces. Two of them, the administrative and mission ones, are obviously externally derived and historically late; they may be the shape of the future but they are not yet (1939) dominant or even particularly ascendant. As for the remaining two, Oliver makes the hypothesis that a major technologic change (metal tools for clearing gardens, page 470) and a major alteration in polity (the substitution of the *pax Britannica* for indigenous warfare, page 423) produced a trend "away from other systems towards a more elaborate, a more time-occupying, and a more potent decision-influencing rank system" (page 466).

After this brief sketch of the contents of Oliver's book, which gives only the smallest clue to its breadth of treatment and treasury of detail, it may come as a surprise to the reader that Oliver, in his preface, regards his own work rather narrowly as descriptive and only in the most peripheral sense theoretical or "related to theory formation." I may be reading things into this book that the author would reject, but I believe that his formulation of the different and somewhat competing ideologies and his attempt to tie them to underlying conditions of technology and social organization represent a high level of theoretical activity.

Although the field work was done almost two decades ago, Oliver casts much of his material against a theoretical field of much more recent vintage. His terminology conforms to that of Homans, Kluckhohn, and Hoebel, among others. Unfortunately, Oliver does not seem to be aware of such work as that of Karl Polanyi, for the Polanyi approach to such concepts as money and value might have added greater clarity to the book. Similarly, the employment of the concept of "redistribution" with its implications and ramifications might have made Siuai economy seem more rational (although not in terms of classical economics!) and less atomistic.

As a final word I would like to say that this is, above all, an exceedingly fine ethnography and one that is certain to be widely used in testing current theories about the emergence of ranking and stratification and the development of political power. Although it does not give great detail on the precise field methods used, it is an excellent job of analytic reporting and well worth recommending to those scientists who, not anthropologists themselves, would like to see a competent ethnographer at work.

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Information Theory in Psychology. Problems and methods. Henry Quastler, Ed. Free Press, Glencoe, 1955. 436 pp. $6.

Information theory has stimulated at least its share of large-scale conferences and symposia. The papers presented at five of these conferences have been collected into book or booklike form, and now we have a sixth.

The conference from which this volume was derived was held under the auspices of the Control Systems Laboratory at the University of Illinois in the summer of 1954. It had a somewhat more restricted scope than, for instance, the biannual London conferences on communication theory, in that it was exclusively concerned with the application of information measures and theory to behavioral studies. However, of the 53 conferees and/or contributors to this volume, 21 are not professional psychologists, or in any event are not listed in the latest directory of the American Psychological Association. Quastler, who organized the meeting and edited this volume of the proceedings, is a radiobiologist.

Partly as a result of the interdisciplinary structure of the meeting, nonpsychologists will find things of interest here. In particular, those who are concerned with the application of information theory to finite samples and situations that do not fit the conventional communication system model will find some of their problems discussed. The admonitions of Cronbach, "On the nonrational application of information measures in psychology," are a particularly good antidote to the overenthusiastic acceptance of the verbalisms of information theory. It probably should be pointed out that the too ready use of information measures is by no means confined to psychologists; other conferences on information theory could well have started out with this same skeptical note.

Also in this volume will be found the first readily accessible publication of Miller's work on the bias of information estimates. In fact, this is the first collection of papers on information theory in which there appears a recurring awareness of the *statistical* problems involved in the application of information measures.

Unfortunately, such genuine contributions to the field are interleaved with a number of other papers that either duplicate material which the same author has presented more completely elsewhere or that read like informal progress reports to a small group of coworkers. Perhaps the best part of the book is the editorial comment by Quastler, who introduces and then comments on each paper. These comments are uniformly wise and thoughtful, and in many instances they contain substantial amounts of additional data. By this device, Quastler gives a coherence to this collection of papers that is more apparent than real. The reader who would like to get a picture of the successes and failures of information theory in psychology would do well to read Quastler's comments first and then make a judicious selection of the papers that constitute the bulk of the volume.

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Handbook of Vital Statistics Methods. Series F, No. 7. *Studies in Methods.* Statistical Office of the United Nations, New York, 1955. 258 pp. $2.50.

The purpose of this handbook is to "explore current practices, to make conveniently available recommended statistical standards, and to present uniform concepts, definitions and procedures upon which development of national vital statistics and the improvement of their international comparability may be based."

Following a historical summary, the whole scope of vital statistics operations is described, including their uses; legal and administrative provisions for their recording; definitions of the events covered (births, deaths, stillbirths, marriages, and divorces); duties and qualifications of registrars and informants; types of registration; details of the reporting process and of report forms; uses, definitions, and classification of statistical items as well as problems relating thereto; compilation and tabulation of data from the reports; computation of rates and indexes; and evaluation of the entire system.

The presentation of the basic principles involved by citation of actual practices in 65 different countries avoids the impression of dogmatic pronouncements and hence makes the recommendations especially acceptable. Moreover, the equitable discussions of the advantages and disadvantages of various methods—for example, "civil" versus "health" administration—serve to suggest adoption of specific arrangements that will avoid or circumvent the deficiencies inherent in a particular system. Similarly, the exposition of the problems created by variations in definitions or in application of definitions—for example, "live birth" versus "stillbirth"—emphasizes the care required in analysis. Authoritative suggestions regarding such basic factors as size, shape, and layout of report forms, color of forms and inks, and wording of instructions bring the report to practical elementals.

Detailed and summary tables have been profitably used to identify general patterns of vital statistics practices. Such factors as the limitations of specific collection methods—for example, aggregation of data from summary reports versus compilation from individual reports—have been readily illustrated by this means. Specific vital statistics data from selected areas also serve effectively to demonstrate the points at issue.

Coding, classification, and tabulating problems are explained in detail, and the need for error control is emphasized. Tabulating methods are briefly but adequately mentioned, while such factors as coverage (partial versus complete), time reference (year of registration versus year of event), and geographic reference (occurrence versus residence) are thoroughly explored. Specific "minimum" tabulations are outlined; in addition, "optional" ones are presented for jurisdictions that can undertake them. Statements of the uses or purposes of each tabulation fully justify the recommendations.

The description of rates, ratios, and other vital indexes is sufficient for a general understanding of the problem of computing them and of the limitations inherent in them.

Evaluation methods include sugges-

tions for testing completeness of registration and of statistical reporting as well as for assessment of the quality of the data. The suggestions are not exhaustive, but they are indicative of the directions evaluation may take.

The *Handbook* is, in effect, a commentary on the 1953 *Principles for a Vital Statistics System* that demonstrates from actual practices the rationale behind the latter document. Its recognition that more than one method may serve the principles laid down enhances its effectiveness.

The annexes include a chronology of important events in registration and vital statistics history, a reproduction of the earlier *Principles of a Vital Statistics System*, definitions of "stillbirth" in 84 countries, and an extremely valuable list of references classified by subject matter. The index is sufficiently detailed to afford ready reference to specific subjects.

Altogether, the *Handbook* provides a liberal education in its field. It should be read by every consumer of vital statistics. For registrars and producers of vital statistics, it is a must, and for areas that are building a vital statistics system, it represents an indispensable *vade mecum*.

CARL L. ERHARDT
Bureau of Records and Statistics, New York City Department of Health

New Books

Swine Production. W. E. Carroll and J. L. Krider. McGraw-Hill, New York, ed. 2, 1956. 496 pp. $6.50.

Microscopic Petrography. Wm. Heinrich. McGraw-Hill, New York, 1956. 296 pp. $6.50.

Physics of Fully Ionized Gases. Lyman Spitzer, Jr. Interscience, New York, 1956. 105 pp. Paper, $1.75; cloth, $3.50.

Economic Geography of Industrial Materials. Albert S. Carlson, Ed. Reinhold, New York; Chapman & Hall, London, 1956. 494 pp. $12.50.

Kernmomente. Hans Kopfermann. Akademische Verlagsgesellschaft, Frankfurt, 1956. 462 pp.

General Biology. Harrington Wells and Patrick H. Wells. McGraw-Hill, New York, 1956. 520 pp. $6.50.

Alcoholism, Its Psychology and Cure. Frederick B. Rea. Philosophical Library, New York, 1956. 143 pp. $3.50.

Life in Fresh Water. E. S. Brown. Geoffrey Cumberlege. Oxford Univ. Press, London. 1955. 64 pp. $2.75.

Voices on the Wind. Polynesian myths and chants. Katharine Luomala. Bishop Museum Press, Honolulu, 1955. 191 pp.

Portraits from Memory. Recollections of a zoologist. Richard B. Goldschmidt. Univ. of Washington Press, Seattle, 1956. 181 pp. $3.50.

The Physical World. Paul McCorkle. McGraw-Hill, New York, ed. 2, 1956. 465 pp. $5.25.

Biochemical Mechanisms in Inflammation. Valy Menkin. Thomas, Springfield, Ill., ed. 2, 1956. 437 pp. $9.50.

A Venture Forward. A history of the American College of Hospital Administrators. Ira A. Kipnis, Comp. American College of Hospital Administrators, Chicago, Ill., 1955. 145 pp.

A Textbook of General Physiology. Philip H. Mitchell. McGraw-Hill. New York, ed. 5, 1956. 885 pp. $10.50.

Culture, Psychiatry and Human Values. The methods of a social psychiatry. Marvin K. Opler. Thomas, Springfield, Ill., 1956. 242 pp. $6.

Every Other Bed. Mike Gorman. World, Cleveland, 1956. 318 pp. $4.

Thinking Things Through. An introduction to logic. Maylon H. Hepp. Scribner's, New York, 1956. 455 pp. $5.50.

The Annual Survey of Psychoanalysis. A comprehensive survey of current psychoanalytic theory and practice. vol. III, 1952. John Frosch, Ed., and Nathaniel Ross, Sidney Tarachow, and Jacob A. Arlow. International Universities Press, New York, 1956. 682 pp. $10.

Art in Coinage. The aesthetics of money from Greece to the present day. C. H. V. Sutherland. Philosophical Library, New York, 1956. 223 pp. $7.50.

The Religion of Negro Protestants. Changing religious attitudes and practice. Ruby Funchess Johnston. Philosophical Library, New York, 1956. 224 pp. $3.

History of American Technology. John W. Oliver. Ronald Press, New York, 1956. 676 pp. $6.50.

Proceedings of the International Conference on the Peaceful Uses of Atomic Energy. vol. 15, *Applications of Radioactive Isotopes and Fission Products in Research and Industry*. 327 pp. $7.50. vol. 16, *Record of the Conference*. 203 pp. $5. United Nations, New York, 1956 (order from Columbia University Press, New York).

Textbook of Chemistry. Edward Mack, Jr., Alfred B. Garrett, Joseph F. Haskins, Frank H. Verhoek. Ginn, Boston, ed. 2, 1956. 854 pp. $6.50.

Handbook of Mammals of Kansas. Misc. Publ. No. 7. Contribution from the State Biological Survey of Kansas. E. Raymond Hall. Museum of Natural History, University of Kansas, Lawrence, 1955. 303 pp. Paper, $1.50; cloth, $4.

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Methods of Biochemical Analysis. vol. III. David Glick, Ed. Interscience, New York, 1956. 437 pp. $9.50.

The Navajos. Ruth M. Underhill. University of Oklahoma Press, Norman, 1956. 299 pp. $4.50.

Frequency Response. Rufus Oldenburger, Ed. Macmillan, New York, 1956. 372 pp. $7.50.

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Le Magnetisme des Corps Célestes, vol. 1, *Magnétisme Solaire et Stellaire Couronne Solaire et Lumière Zodiacale*; vol. 2, *Variations et Origine du Géomagnétisme*, pts. II and III of *Physique Cosmique*, A. Dauvillier (Hermann), 8 July 1955, 81

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Radio Astronomy, J. L. Pawsey and R. N. Bracewell (Oxford Univ. Press), 23 Dec. 1955, 1237

Transactions of the International Astronomical Union, P. Th. Oosterhoff, Ed. (Cambridge Univ. Press), 10 June 1955, 827

Variable Stars and Galactic Structure, C. Payne-Gaposchkin (Athlone), 19 Aug. 1955, 338

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Adrenal cortex, E. P. Ralli, Ed. (Josiah Macy, Jr., Foundation), 28 Oct. 1955, 835

Advances in Enzymology and Related Subjects of Biochemistry, F. F. Nord, Ed. (Interscience), 6 May 1955, 669

Advances in Protein Chemistry, vol. IX, M. L. Anson, K. Bailey, J. T. Edsall, Eds. (Academic), 19 Aug. 1955, 336

The Alkaloids: Chemistry and Physiology, R. H. F. Manske and H. L. Holmes, Eds. (Academic), 29 Apr. 1955, 637

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Biochemical Investigations in Diagnosis and Treatment, J. D. N. Nabarro (Little, Brown), 15 Apr. 1955, 565

Biochemistry and Physiology of Protozoa, vol. II, S. H. Hutner and A. Lwoff, Eds. (Academic), 26 Aug. 1955, 380

Biochemistry of the Aminosugars, P. W. Kent and M. W. Whitehouse (Academic), 16 Sept. 1955, 520

Biochemistry of the Developing Nervous System, H. Waelsch, Ed. (Academic), 6 Apr. 1956, 591

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The Chemistry of Lipids of Biochemical Significance, J. A. Lovern (Wiley), 14 Oct. 1955, 696

Ciba Foundation Symposium on Chemistry and Biology of Pteridines, G. E. W. Wolstenholme and M. Cameron, Eds. (Little, Brown), 26 Aug. 1955, 381

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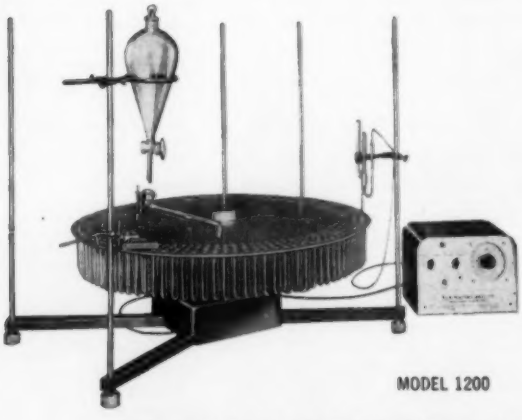

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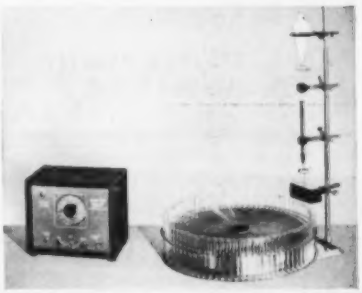

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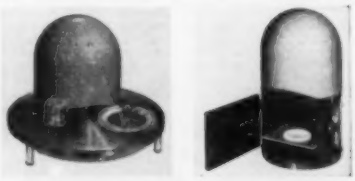

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Treatise on Invertebrate Paleontology, R. C. Moore, Ed. (Univ. of Kansas Press), 17 June 1955, 857

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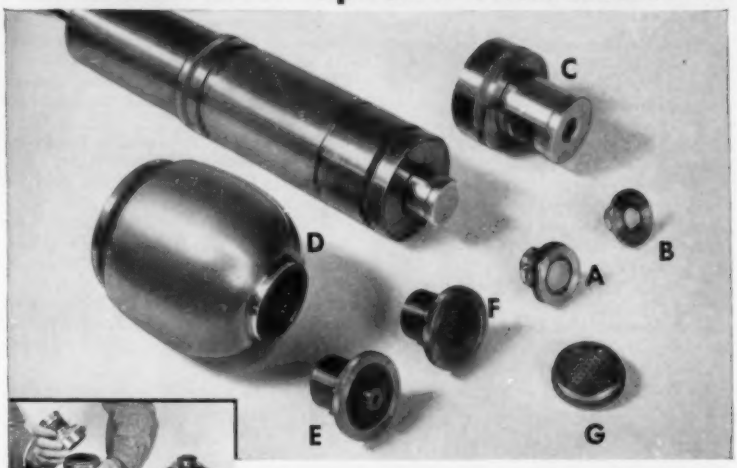

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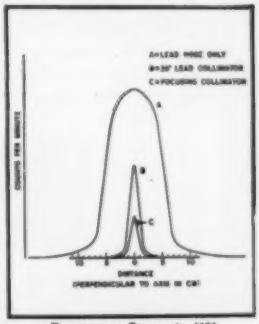

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25–27. Soc. for Applied Anthropology, annual, Boston, Mass. (Mrs. E. Purcell, 150 E. 35 St., New York 16.)

26–28. International Cong. of Assoc. for Study of Bronchi, 6th, Brussels, Belgium. (Dr. Maunes, 23 Ave. d'Anderghem, Brussels.)

28–30. Chemical Inst. of Canada Conference, 39th annual, Montreal, Que. (Chemical Inst. of Canada, 18 Rideau St., Ottawa 2, Ont.)

28–1. International Symposium on Venereal Diseases and the Treponematoses, 1st, Washington, D.C. (C. A. Smith, Venereal Disease Program, Div. of Special Health Services, Public Health Service, Washington 25.)

28–2. International Dental Federation, 44th annual, Zurich, Switzerland. (H. Freihofer, 15 Todistrasse, Zurich.)

30–9. International Conf. of Large Electric Systems, 16th, Paris, France. (J. Tribot Laspiere, 112 Blvd. Haussmann, Paris 8e.)

June

3–6. American Soc. of Refrigerating Engineers, Cincinnati, Ohio. (R. C. Cross, ASRE, 234 Fifth Ave., New York 1.)

3–7. Special Libraries Assoc., annual, Pittsburgh, Pa. (Miss M. E. Lucius, 31 E. 10 St., New York 3.)

3–8. Soc. of Automotive Engineers, summer meeting, Atlantic City, N.J. (Meetings Div., SAE, 29 W. 29 St., New York 18.)

4–7. Forest Products Research Soc., Asheville, N.C. (F. J. Rovsek, FPRS, P.O. Box 2010, University Station, Madison 5, Wis.)

4–8. American Soc. of Civil Engineers, national conf., Knoxville, Tenn. (W. H. Wisely, ASCE, 33 W. 39 St., New York 18.)

4–9. International Mechanical Engineering Cong., 6th, Paris, France. (British Engineers Assoc., 32, Victoria St., London, S.W.1, England.)

4–9. International Seed Testing Convention, Paris, France. (C. Stahl, International Seed Testing Assoc., Thorvaldsensvej 57, Copenhagen V, Denmark.)

4–9. Microbiological Inst., 9th annual, Lafayette, Ind. (Div. of Adult Education, Engineering Administration, Bldg., Purdue Univ., Lafayette.)

6–8. American Soc. for Quality Control, annual, Montreal, Quebec, Canada. (C. E. Fisher, ASQC, Room 563, 50 Church St., New York 7.)

6–9. European Federation for Chemical Engineering, 9th, Frankfurt/Main, Germany. (Dechema-Haus, Rheingau-Allee 25, Frankfurt A.M.)

6–10. American College of Chest Physicians, annual, Chicago, Ill. (M. Kornfeld, ACCP, 112 E. Chestnut St., Chicago.)

7–9. Endocrine Soc., annual, Chicago, Ill. (H. H. Turner, 1200 N. Walker St., Oklahoma City, Okla.)

9–10. Soc. for Investigative Dermatology, annual, Chicago, Ill. (H. Beerman, 255 S. 17 St., Philadelphia 3, Pa.)

10–14. Institute of Food Technologists, annual, St. Louis, Mo. (C. S. Lawrence, IFT, 176 W. Adams St., Chicago 3, Ill.)

10–15. American Crystallographic Assoc., French Lick, Ind. (S. Siegel, Chemistry Div., Argonne National Lab., Box 299, Lemont, Ill.)

11–15. American Medical Assoc., annual, Chicago, Ill. (G. F. Lull, AMA, 535 N. Dearborn St., Chicago 10.)

11–15. Symposium on Molecular Structure and Spectroscopy, annual, Columbus, Ohio. (H. H. Nielsen, Dept. of Physics, Ohio State Univ., Columbus.)

11–16. Pacific Div., AAAS, Seattle, Wash. (R. C. Miller, California Acad. of Sciences, Golden Gate Park, San Francisco 18.)

11–23. European Organization for Nuclear Research, Symposium on High Energy Physics, Geneva, Switzerland. (H. Coblans, CERN, Case Postale 25, Genève 15-Aéroport.)

12. National Assoc. of Science Writers, annual, Chicago, Ill. (M. L. Silverman, NASW, 4 Bassett Lane, Atherton, Calif.)

12–14. American Meteorological Soc., Seattle, Wash. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

12–15. Max Planck Soc. for Advancement of Sciences, Stuttgart, Germany. (Max Planck Soc., Raiserswerther Str. 164, Düsseldorf 22a, Germany.)

12–16. World Conference on Earthquake Engineering, Berkeley, Calif. (R. W. Clough, Div. of Civil Engineering, Univ. of California, Berkeley 4.)

13–14. Conference for Veterinarians, 25th annual, Columbus, Ohio. (J. W. Helwig, College of Veterinary Medicine, Ohio State Univ., Columbus 10.)

13–16. Colloquium of College Physicists, annual, Iowa City, Iowa. (G. W. Stewart, Dept. of Physics, State Univ. of Iowa, Iowa City.)

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(See issue of 20 April for comprehensive list)

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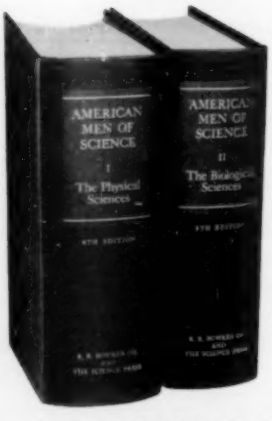

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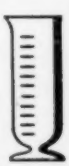

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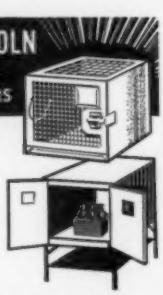

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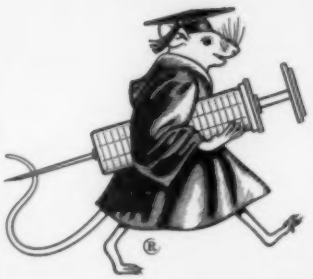

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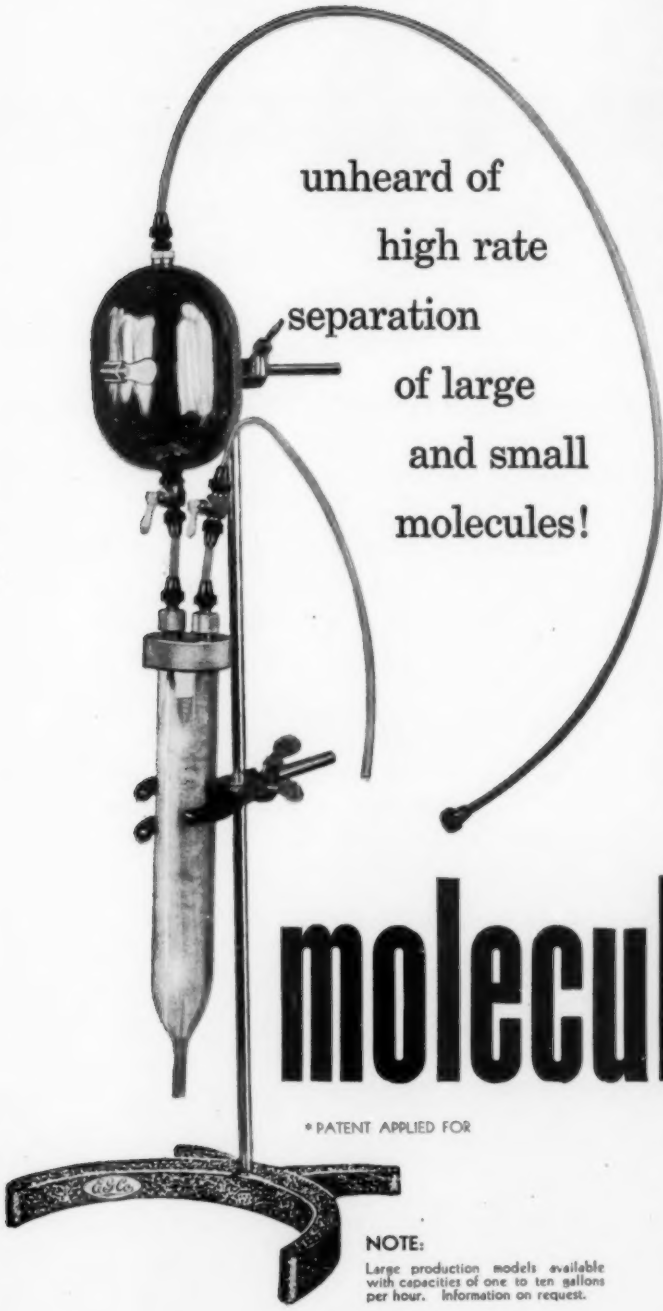


unheard of high rate separation of large and small molecules!

Today, laboratories can attain molecular filtration of up to 250 ml. per hour of water . . . more than 100 times the rate possible with any previous type of laboratory equipment!

These unheard of rates of filtration are accomplished by using a Molecular Filter*, invented by Dr. Clarence Hiskey of the International Biochemical Co. It utilizes ordinary pressure filtration methods to separate large and small molecules directly from solution. Now any laboratory technician with ordinary pressure filtration training can achieve high rate separation of large and small molecules.

In addition to being faster and simpler to operate, the new Molecular Filter has easily replaceable filter cartridges, utilizes hand connections that require no special tools, and permits continuous or intermittent operation. It has high throughput and low holdup. It is unitized for immediate use.

Detailed description The apparatus consists of a stainless steel reservoir and filter unit mounted on a stand. Solution to be filtered is introduced to the reservoir having a capacity of 1750 ml. A pressure of 100 lbs. per square inch is communicated to the reservoir through saran tubing and fittings that are tightened by hand. A toggle valve controls the flow of solution from the reservoir to the inlet end of the filter cartridge contained in a pyrex cell. Special convenient fittings are used to connect the cartridge to the reservoir and cell. Filtrate passes through the walls of the cartridge and collects in the cell where it can be removed from the outlet at its bottom. Residue is removed from the outlet end of the cartridge where another toggle valve is supplied for control.

The replaceable cartridge filter (a 125 and a 250 ml./hour unit are supplied) contains a cellulosic tube approximately ¼" in diameter with a wall thickness of 1½ thousandths of an inch. This extremely thin membrane, with pores of approximately 5 millimicrons, is supported by a white cotton braid along its full length. The braided element is coiled and folded to fit in a nylon bag to act as a support and limit the amount of holdup.

molecular filter

* PATENT APPLIED FOR

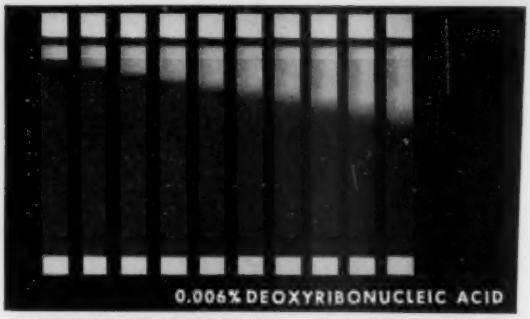
NOTE:
Large production models available with capacities of one to ten gallons per hour. Information on request.

PRICE LIST

G8275	Molecular Filter, International Biochemical, complete with tank, valves, fittings, glass cell, and set of two cartridges but without stand and clamps	ea. 278.45
G8275 A	Complete set of clamps and stand for G8275	ea. 18.55
G8275 M	Filter cartridge, only, 125 ml./hr. size for replacement in G8275	ea. 4.50 lots of 6 ea. 4.00
G8275 N	Filter cartridge, only, 250 ml./hr. size for replacement in G8275	ea. 5.75 lots of 6 ea. 5.00

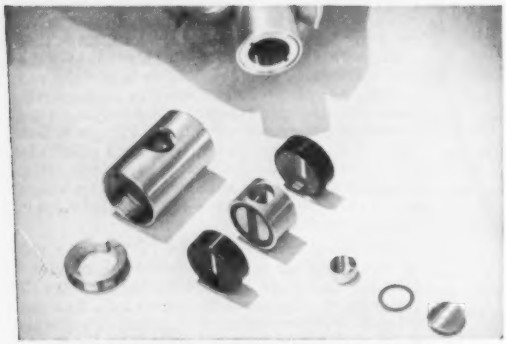
20-26 N. Moore Street, Dept. 238, N.Y. 13, N.Y. The EMIL GREINER Co.

RAYLEIGH INTERFERENCE FRINGES provide high-sensitivity measurement of solute concentration at any level in analytical cell. Used for determining molecular weights—both high and low—by the equilibrium method, as well as for measuring sedimentation velocity by the centrifugal transport of material past a given level in the cell.



ULTRAVIOLET ABSORPTION method gives very high sensitivity with respect to concentration rather than concentration gradient, as in schlieren techniques. Newly-designed optical system is particularly valuable for such materials as nucleoproteins and other ultraviolet-absorbing substances exhibiting diffuse or inhomogeneous boundaries.

HOW new developments have increased the versatility of **Analytical Ultracentrifugation**



SYNTHETIC-BOUNDARY cell permits establishment of an immediately-measurable boundary by a layering technique. This extends sedimentation-rate measurements from the previous practical lower molecular-weight limit of 10,000, downward to 500.

40,000-rpm SWINGING-BUCKET rotors make preparative operations most closely comparable with analytical, give most accurate estimation of sedimentation by sampling. Boundaries sediment normally and undergo minimum reorientation. Quartz inserts permit spectrography.

Since 1947, when it was introduced, the Spinco Model E Analytical Ultracentrifuge has been constantly broadened in utility by new developments in equipment and technique.

Besides the specific items illustrated above, provisions have been made for: (1) simultaneous analysis of two materials in the same rotor; (2) temperature measurement and continuous, automatic control of operating rotors to better than 0.1 degree C from zero to 100 degrees C; and many other specialized functions.

Each of these developments has been engineered to permit its application to any Spinco Model E in existence. Your inquiry is invited for further information on these or other developments which may provide solutions to your problems in physical research.

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Spinco *division*

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